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Original Communications.

*ON SUPERABUNDANT ALIMENTATION, AND ON FORCED FEEDING; AND ON THE DIET IN GOUT, URIC ACID, AND BILIOUS LITHIASIS.**

By Dr. Dujardin-Beaumetz, Paris, France.

GENTLEMEN:—In the last lecture I considered the subject of insufficient or spare diet, and showed its application to the treatment of obesity. To-day I shall take up the opposite aspect of the subject, namely, the effects of superalimentation, and the therapeutic advantages occasionally derivable therefrom.

A diet may be rich in one or all of the alimentary substances described under the head of complete and complex aliments. Sometimes it is the azotized substances which are superabundant, sometimes the amylaceous, sometimes the fatty; drinks may be taken in too great quantity. In this lecture I shall consider chiefly that side of the question which pertains to superabundant azotized alimentation, and its applications to therapeutics, closing with some observations respecting the transformation of azotized matters in the economy.

Superalimentation has been applied to the

* An abstract of two lectures on Hygienic Therapeutics. From advanced sheets.

treatment of certain pathological states, such as consumptive complaints and leanness from whatever cause. Fattening in man is subjected to the same laws as those which have been established by zootechnics. To fatten animals three conditions are necessary: they must be of a certain breed, they must be stabled or otherwise subjected to confinement, and they must be fed with certain kinds of food. As now there are varieties or breeds of animals which fatten with greater or less difficulty, so also in the human species there are breeds as well as individuals which resist all efforts to fatten them. There are also fat men who eat very little, and there are lean men who are enormous eaters; and in the case of leanness as well as of obesity, heredity plays a great part, leanness and stoutness running in families.

But there is a factor which renders difficult the application of the laws of zootechnics to man; I refer to the fact, which I have mentioned before, that brain-work is a cause of denutrition even greater than muscular labor. Hence if we should condemn a man to absolute physical rest, we might not thereby stop all mental activity, and the organic combustions and denutrition which follow. I cannot, then, accept the general statement of Brillat-Savarin: "Everybody can become fat, provided the proper kind of diet be chosen."

Moreover, the pathogeny of leanness is very complex. Gubler distinguishes three kinds, basing his classification on pathogeny: that which results from insufficient food,—starvation leanness; that which results from excessive waste of the tissues, due to the exaggerated combustions of the economy,—consumption leanness; and that which is of a hereditary nature,—constitutional leanness. You readily understand that the therapeutic results which you are to expect will depend more on the cause than on anything else, hence your first care should be to ascertain the circumstances under which the patient's loss of flesh has arisen.

The treatment of leanness comprehends, then, like that of obesity, a pharmaceutical and a hygienic treatment. I shall say but little about the former. Certain remedial agents have been vaunted; it has been claimed that metals, such as mercury and copper, possess fattening properties. Wyman has pretended that fusel oil (or potato spirit) has such properties in the dose of from 5 to 10 drops. This is by no means proved, and there is but one medicament that can be clearly shown to cause fattening, and that is arsenic. The popular usage which the inhabitants of Lower Austria, Styria, and Carinthia make of arsenic is certainly in conformity with the physiological effects of the arsenical preparations, which stimulate nutrition in general and increase the appetite. In the treatment of leanness as well as of obesity, hygiene plays the principal part; under this head we are to consider exercise and alimentation.

As for exercise, the less of it the better, is the general rule for those who wish to gain flesh, and it is worthy of remark in this connection that the idle and luxurious life to which the women in Turkey are condemned (I allude especially to those in upper life) accounts sufficiently for the *embonpoint* which characterizes them.*

It is alimentation, however, which has the chief rôle in adipose tissue development and increase, and when called upon to treat cases of emaciation, your first duty will be to order foods rich in fat and hydrocarbons. In the first rank comes cod-liver oil, which is always to be ordered when it can be borne, then fat meats, cream, and carbo-hydrates are to be used largely as parts of the daily fare. The following table from De Nedats will be useful as giving you the richness in starch of certain alimentary substances from the vegetable kingdom. Below is a second table giving the percentage of fat in the principal fatty foods:

TABLE OF AMYLACEOUS SUBSTANCES.

	Per cent.
Rice	74.10
Indian corn	65.90
Wheat flour	63.00
Unground wheat	59.60
Millet	57.9
Buckwheat	50.00
Wheat bread	42.70
Peas	37.00
Rye bread	36.25
Beans	36.00
Cucumbers	16.60
Potatoes	15.5

TABLE OF FATTY SUBSTANCES.

	Per cent.
Butter	91.00
Fresh bacon	66.00
Pork	50.00
Mutton	40.00

* Meibomius counsels flagellation! as a fattening measure; "it produces," he says, "swelling of the flagellated parts, and causes derivation thither of the nutritive juices!"

	Per cent.
Cooked beef	44 79
Raw beef	30.00
Yolk of egg	30.70
Cream	26.00
Neufchâtel cheese	41.00
Holland cheese	27.50
Parmesan "	16.00
Brie "	25.70
Eel	13.80
Salt herring	12.70
Eggs	12.50

While in the treatment of obesity soups and stews are forbidden, here they are prescribed. It is well, also, to order much water to be taken with meals. But, I repeat, despite all the care with which you may direct the treatment of leanness, you will often fail, and for reasons which I have mentioned.

In zootechnics, besides the application of the general precepts on the selection of food and the feeding of animals, stuffing is sometimes practised, as is the case with fowls that are to be fitted for market at a given time. For this purpose, besides the ordinary process of cramming down the throat an alimentary paste, certain apparatuses are much in use, such as the Martin-stuffer, an instrument resembling somewhat the "gavense" of our hospitals.

These methods of gavage we have also applied to man. Two circumstances have seemed to call for gavage: when the individuals refuse to eat, and we have to resort to *forced feeding* in order to save life, or when the stomach rejects food given in the ordinary way, but retains that which is administered by gavage. In cases of the first kind forced feeding is applied to demented persons who are affected with delusions that they are dead, that they have no mouth, stomach, intestine, etc. In order to practise forced alimentation, many devices have been tried to prevent the patient biting with his teeth through the œsophageal tube. The most simple procedure is to pass the sound through the nares. By this tube (which must, of course, be of small calibre) you can easily introduce a thin gruel or a mixture of milk and meat-powder with wine. The meat-powders here render us great service, for their state of cohesion is such that we can readily make of them a nutrient broth which shall be liquid enough to pass through quite small tubes. You will find in the annals of mental alienation a great many cases where life has been prolonged, and even cures effected in melancholic patients, by means of forced alimentation.

Gavage, properly so called, is applied to cases where the stomach is intolerant and rejects food ingested by the mouth. Since the labors of Debove, and since my own researches, it has been shown again and again that in many circumstances aliments which are introduced directly into the stomach are tolerated, while food swallowed by the patient is rejected by vomiting. In the vomitings of tuberculosis, of pregnancy, in such as are unmistakably of nervous origin, we have been able to feed the patient through the Debove tube. You know that in these cases it is not necessary to pass the tube down into the stomach, for all the purposes of artificial alimentation are accomplished if the end of the tube is made to penetrate only as far as the upper part of the œsophagus. To-day, thanks to cocaine, the passage of the tube is made with great facility.

I once devised a stomach-sound of my own, which I made much use of; but I have had occasion to abandon it for the new Debove stomach siphon made by Galante. I employ only the smaller-sized tubes, and find them both stiff enough and supple enough for the purpose. It is not needful, you understand, to resort to gavage except in cases of vomiting or of total loss of appetite.

When the patient can eat, and with some relish and willingness, you can have recourse to superalimentation without employing gavage. When Debove and I commenced our experimentation on meat-powders we lacked the improvements which have since been made in them. We then used stomach-tubes more than we do now. To-day, save in the special cases which I have mentioned, you can administer the meat-powder directly to your patients. This alimentary preparation, by reason of its high nutritive value in small bulk, is admirably adapted for superalimentation. You may employ mixtures of lentil flour or meat-powder in the form of broth, or you may give the meat-powder alone with chocolate (which constitutes quite an agreeable beverage), or you may incorporate it with milk-punch in the following way: In an empty bowl place a couple of tablespoonfuls of meat-powder; add one or two tablespoonfuls of old rum or Bourbon and half a pint of milk, sweetened *ad libitum*. As for the quantity of meat-powder, you may begin with 100 grammes a day, and gradually increase to 200, 300, and 400 grammes, which may be taken in divided portions during the day.

You all know the remarkable results which have been obtained from superalimentation in the treatment of consumptive diseases.

You will find cases of extraordinary interest recorded in articles by Debove and his pupils, Broca and Wims. I also refer you to published reports of my own and the recent memoir of my interne, Pennel.* You obtain a marked augmentation of weight, a general amelioration, and in certain rare cases an arrest in the march of the tuberculosis. All these results have been confirmed in Germany at the clinic of Griefswald by Peiper, who has shown us that by administering to phthisical patients meat-powder in quantities gradually increasing from 200 to 500 grammes a day, a striking augmentation of weight has been observed, amounting from five to twenty-two pounds in twelve patients out of every fourteen. In one patient the clinical clerk failed any longer to find bacilli in the sputum, and physical examination showed a diminution in the local lesions. This man had gained seventeen and one-half pounds in thirty-four days. Superalimentation has, then, proved itself of the greatest utility in phthisis, and must henceforth have an important place in the therapeutics of this disease.

Now that you know the applications of superalimentation to the treatment of consumptive affections, and particularly to that of tuberculosis, I approach one of the most interesting points connected with azotized diet,—namely, the transformation of azotized substances in the economy and their elimination in the state of urea and uric acid. These two bodies play so important a part in the genesis of gout and gravel that I find myself under the necessity of summing up in a few words what we know as to the nature and origin of these effete products of the organism.

Urea is, as you know, represented by the following atomic formula: CON_2H_4. It is extracted directly from urine, or obtained by synthesis; and you are all familiar, in this regard, with the brilliant achievements of my regretted master, Wurtz, who made urea out of cyanate of ammonium. Béchamp has even claimed that, by the direct oxidation of albuminoid matters by means of potassium permanganate, urea might be obtained.

Urea constitutes the basis of a series of complex bodies to which has been given the name of the *uric acid series*. It includes compound ureas and ureides; and for more particulars on the subject I cannot do better than refer you to the thesis on Ureides of the regretted Henninger.†

In a general way, it may be said that all the chemists are agreed in considering urea as an amide; it is an amide with carbonic acid, or *carbamide*. I need not here remind you of what is understood in chemistry by an *amide;* you know that it is an ammoniacal salt minus a certain quantity of water. The formula which I place before you shows that in abstracting from neutral carbonate of ammonium two atoms of water you have the formula of urea:

$$CO<\begin{matrix}ONH_4\\ONH_4\end{matrix} - 2H_2O = CO<\begin{matrix}NH_2\\NH_2\end{matrix} = CON_2H_4.$$

Neutral carbonate of ammonium. — Water. — Carbamide, or urea.

Urea is found in all the tissues of the economy, and a man secretes on an average twenty to thirty grammes a day. We shall see farther on the circumstances which augment or diminish this secretion.

Like urea, uric acid (whose formula is $C_5H_4N_4O_3$) is found in the normal state in the urine, but in much less considerable quantity, since in the twenty-four hours the normal excretion in an adult man is only 0.58 gramme. Uric acid forms with bases neutral and acid salts; the most abundant of these salts in the economy is the acid urate of sodium, which constitutes brick-dust sediments in the urine, and with whose varying microscopic appearances you are familiar: daggers, halberds, lozenges, etc.

Many views have been put forth as to the origin of urea and uric acid. The first, and which till within a few years has been the prevalent view, is that urea and uric acid are the result of the oxidation of albuminoid substances. When the oxidation is complete there is formation of urea; when it is incomplete it is uric acid which is formed: urea and uric acid being the waste products of organic combustion. This view was founded on both chemical and physiological experiments.

Wöhler and Liebig, in their great work bearing date 1838, had shown that uric acid, in undergoing oxidation, is transformed successively into allantoin, alloxane, and urea.

* Debove, "Sur le traitement de la phthisie pulmonaire par l'alimentation forcée" (*Bull. de Thérap.*, 30 Novembre, 1881); Dujardin-Beaumetz, "Sur nouveau procédé de gavage" (*Bull. de Thérap.*, 15 Juillet, 1881; *Clinique thérapeutique*, Du Gavage, 4e édition, t. i. p. 404); Broca et Wims, "Recherches sur la suralimentation envisagée surtout dans le traitement de la phthisie pulmonaire" (*Bull. de Thérap.*, 1883, t. cv. p. 289); Pennel, "De l'alimentation chez les phthisiques" (*Bull. de Thérap.*, 1882, t. cii. p. 185); Peiper, "De l'alimentation forcée des phthisiques" (*Deut. Arch. f. Klin. Med.*, 1885, vol. xxxvii.).

† Henninger, "Des Uréides" (*Thèse de Concours*, 1878).

On the other hand, Béchamp, in demonstrating that the direct oxidation of albuminoid matters furnishes urea, had given to this view an experimental proof which seemed indisputable. Experiments made on men and on animals seemed to confirm this position. In examining the quantity of urea and uric acid excreted during the periods of repose and of work, it was shown that there exists between the quantity of urea voided in twenty-four hours and that of uric acid an inverse proportion, and that while during rest the quantity of urea is inconsiderable, that of uric acid is large; conversely, muscular labor causes an increase in the quantity of urea and a decrease in that of uric acid. Ritter's table, which I here place before you, shows this conclusively:

	Quantity of urine.	Total of nitrogen.	Ammonia.	Urea.	Uric acid.
Rest.....	1340 grms.	17.89	0.48	32.90	0.98
A walk of four hours.....	1940 grms.	20.00	0.62	39.25	0.68
A march of four days.....	2120 grms.	20.30	0.59	40.30	0.62

The influence of regimen in the production of urea and uric acid also comes to the support of this view, for the quantity of both of these principles augments with an azotized diet, and diminishes with a vegetable diet and with abstinence.

As for urea the fact is not doubtful, and it is the same with uric acid. Lehmann, in experimenting on himself, found in twenty-four hours the following figures pertaining to the uric acid excretion:

An animal diet.....	1.47 grammes.
A mixed diet.....	1.18 "
A vegetable diet.....	1.02 "

Ranke* has noted still more marked differences, as the following table shows:

An animal diet.....	0.88 gramme.
A vegetable diet.....	0.65 "

But certainly the most interesting experiment is that which Boussingault has performed on ducks fed on different substances, and the results to which he arrived are as follows:

DAILY AMOUNTS OF URIC ACID EXCRETED.

	Grammes.
During entire abstinence from food.....	0.27
After ingestion of balls of clay.....	0.27
" " gum arabic.....	0.29
" " cheese curd.....	10 55
After ingestion of gelatin.....	10.21
After a new dose of gelatin.....	13.21
Under the influence of a diet of fibrin...	9.10
" " " " flesh...	18.91

Lastly, some direct experiments made by Frerichs and Wöhler had showed that in giving dogs uric acid the sum of urea excreted is increased, and the researches of Stockvis, of Zabelin, of Neubauer, are confirmatory of these experiments.†

The theory which would assign a common origin to urea and uric acid seemed then absolutely demonstrated, and had become generally accepted; it considered uric acid as the incompletely oxidized remnant of an intermediate body, which was destined in the organism of mammals for a higher degree of oxidation. You will now see that another theory has arisen by the side of this one which teaches that urea and uric acid have each a distinct origin.

As for urea, everybody is agreed in considering it as the result of a breaking up of albuminoid matters, whether the latter come from food or from tissue-waste. The experiments of Panum, of Hugounencq, of Darier, those still more recent of Quinquaud,‡ show the intimate relation which exists between urea production on the one hand and an azotized diet on the other. But new conceptions arise of the metabolisms taking place as we push still further the study of urea formation.

Abandoning the theory of Liebig and Wöhler, who taught that oxygen produces the combustion of albuminoid matters and their direct transformation into urea, Shultzen and Nencki§ have maintained that urea results from successive modifications effected in amide acids, glycocol, leucine, and tyrosine.

* Ranke, "Beob. und Versuch über die Aussch. des Harns." Munch n, 1868.

† Zabelin, *Ann. der Çhem. und Pharm.*, Bd. lxv., s. 335; Neubauer, ibid., supplém. iii., s. 326; Wöhler et Frerichs, "Modifications que diverses substances éprouvent en passant dans l'urine" (*Journ. für Prakt. Chem.*, 1848, t. lxiv. p 60).

‡ Panum, "De la courbe de la sécrétion de l'urée et de l'urine pendant vingt-quatre heures après un repas consistant en une certaine quantité de viande" (*Nordisk Med. Archiv.*, vol. vi. n° 1874); Hugounencq, "Expériences nouvelles sur le dosage de l'azote et la production physiologique de l'urée" (*Thèse de Montpellier*, 1883); Darier, "Recherches cliniques et expérimentales sur les variations de l'urée" (*Rev. med. de la Suisse romande*, t. iii. 65 et 121, Février et Mars, 1883); Quinquaud, "Oscillations et formation de l'urée pendant la digestion des aliments azotés" (*Bull. de la Société de Biologie*, 11 Octobre, 1884).

§ Shultzen et Nencki, "Die Vorstufen des Harstoffs" (*Zeits. f. Biologie*, viii.).

In their estimation, albuminoid substances behave in the following manner in the economy: a portion is destroyed by digestion, another portion, the more considerable, is decomposed in the lacteals and portal circulation into bodies exempt from nitrogen, and into amide acids; the latter are transformed into urea, while the former are burned in the organism and pass out in the form of carbonic acid and water.

Schultzen has undertaken a great many experiments on animals and on man to show the direct transformation of glycocol and of methyl-glycocol, or sarcosine, into urea. These experiments, repeated by Küssner and Salkowski, appear to be demonstrative of the facts advanced by Schultzen and Nencki.

Knierim has put forth another opinion as to the origin of urea; he maintains that urea is not derived from amide acids, but from aspartic acid, and from asparagine.

The same want of agreement is seen when attempts are made to specify the place where this transformation is effected. In the opinion of the majority of physiologists it takes place in all parts of the economy, wherever molecular changes are going on. Nevertheless, the liver has of late years been regarded as the chief centre of urea production, and we see Murchison and Parkes in England, Meissner in Germany, and Brouardel in France, stoutly defend the doctrine of the hepatic localization of urea-genesis, a view which seems to be confirmed by the recent experiments of Von Schroeder,* who, in applying the method of local circulations to the solution of the problem, has shown the production of urea by the hepatic gland. I set aside as not confirmed another hypothesis which has been made, and which assigns to the kidney the function of urea formation, and I come now to recent views which have been set forth as to the origin of uric acid.

Bencke, Voit in Germany, and Lecorché in France, have maintained that uric acid is not a derivative of urea, but of xanthine.

In fact, if you will cast your eyes over the following formulæ, you will notice that hypoxanthine, or sarkine, xanthine, and uric acid differ only by one equivalent of oxygen, and it is easy to see that the last of these bodies might easily arise from oxidation of the two former:

Hypoxanthine, or sarkine, $C_5H_4N_4O$.
Xanthine, $C_5H_4N_4O_2$.
Uric acid, $C_5H_4N_4O_3$.

* Von Schroeder, *Arch. f. Experim. Path. und Pharm.*, Bd. xvi., Heft 5 and 6.

As for the part of the economy where the production of uric acid takes place, there are numerous hypotheses which I shall only mention. Ebstein supposes the marrow of bones to be principally concerned in uric acid genesis, Chrzonsczewski refers this function to the connective tissue, Robin to the fibrous tissue, Ranke to the spleen, Bencke to the white globules, Zaleski to the kidneys, and Meissner and Lecorché to the liver.

I am, myself, inclined to hold to the old view of Wöhler and Liebig as to the origin of urea and uric acid in the economy, and believe that this is the only view having a definite bearing on therapeutics and sanctioned by practice. Therefore I regard urea and uric acid as having a common origin, resulting from the oxidation, more or less complete, of the albuminoid matters of the economy, and I will give you the proofs of this when I come to the dietary of gout. If I have dwelt at length on this subject, it is to call your attention to the numerous works which have appeared thereon, and the importance which in biological chemistry is attached to the study of the metamorphosis of azotized matters. I deemed it also of importance that you should be acquainted with what is known of the origin of these two bodies, urea and uric acid, which by their presence in the economy constitute the uric diathesis, which is so common, and which offers a favorable soil for the development of gouty and rheumatic affections. We shall see numerous applications of the details into which I have entered when we come to the next lecture, in which I propose to speak of the hygienic treatment of gout and gravel.

LECTURE II.

ON DIET IN GOUT, URINARY AND BILIOUS LITHIASIS.

In the preceding lecture I have treated at some length of superalimentation in its effects on the transformation of azotized aliments in the economy, and on the various theories which have been put forth as to the origin of urea and uric acid. I shall have no more to say on these points, and shall now take up the interesting study of diet in the treatment of gout and gravel, affections which result from the accumulation of urea and uric acid in the organism.

In this lecture, concerned exclusively with hygienic therapeutics, I cannot devote much time to the history of gout, or to the numerous discussions which have arisen between the

solidists and humorists with regard to the pathogeny of gout. But I will endeavor to sum up in a few words the recent hypotheses which have been put forth on this subject.

When Garrod published his treatise on gout there was general unanimity in considering it as giving the clearest explanation of the phenomena observed. According to Garrod, gout results from two pathogenic elements,—uricæmia on the one hand, and impermeability of the kidney on the other.

Uricæmia is due to the abundance of urea in the different humors of the economy, and this excess results either from an alimentation too highly azotized, or from insufficient oxidation of albuminoid matters. As for renal impermeability, when existing, it prevents the removal of the effete products of combustion, and thus favors their accumulation in the economy.

This doctrine has not been accepted by all, and to-day there are two quite opposite theories to explain the pathogeny of gout. According to some, as Bouchard and Bencke, gout is the consequence of a failure in the elaboration of albuminoids, and depends on that state of the economy to which they have given the name of *retarded nutrition*. According to others, however, as Lecorché, it is exaggeration in the functions of nutrition which causes the uric diathesis. Let us briefly examine these opposing theories.

Comparing uric acid with other acids of the economy, Bouchard refers gout to the uric diathesis, and endeavors to show that this acid diathesis depends on too slow a destruction of organic substances. Whence come these acids? They result either from the nature of the food ingested, or from vicious or abnormal fermentations taking place in the digestive tube; vicious fermentations being especially favored by dilatation of the stomach. Hence Bouchard, logical with himself, assigns to dilatation of the stomach a preponderant rôle in the pathogeny of gout.

When once formed, these organic acids are eliminated at different points: the sweat gives issue to formic, valerianic, butyric acids; cholalic acid is removed by the intestine, and uric, hippuric, and oxaluric acids by the kidneys. But let some circumstance intervene to modify the play of these different emunctories, these acids accumulate in the economy, and gouty manifestations appear. Quite different is the theory of Lecorché.*

Adopting the new views which give to the cell the preponderant rôle in the dissociation of albuminoid elements, views which I have set forth in the last lecture, Lecorché thinks that gout results from exaggerated dissociation of azotized matters, due to inordinate activity of the organic cells; in a word, hypernutrition engenders gout.

Whether gout be the result of exaggeration or of retardation in the functions of the economy, everybody recognizes that the augmentation in the blood of uric acid is the essential cause of gout, and that its passage to the state of biuret determines the gouty paroxysms. The same agreement exists from the point of view of the influence of diet on the prophylaxis of gout, and it is the dietetic treatment of this diathesis which I am going to speak of in this lecture.

In the prophylactic and curative treatment of gout, authorities have maintained the advantage of exclusive regimens, and vegetarians have endeavored to show that by means of entire abstinence from purely azotized aliments gout might be made to disappear; but an exclusively vegetable diet has its disadvantages, as Sydenham was one of the first to point out. Therefore it is the better practice to give to gouty persons a mixed diet, and this shall be the text of what I have to say to-day. I shall take as my basis Bouchardat's important work on the treatment of what he calls *the polyuric diathesis*,—*i.e.*, a diathesis characterized by excess of production or insufficiency of elimination of uric acid, or of the biurates. Let us examine, then, the alimentary hygiene of the gouty under the several heads of foods, drinks, time for meals, and exercise.

As for azotized aliments, all kinds of meat may be used, predominance being given to white meats over those which are too highly azotized, such as venison or wild fowl. Be not too free in the use of eggs, fish, oysters, lobsters; old cheese is to be avoided. Fatty foods are to be eaten sparingly.

Vegetables generally are to be permitted, and should have a large place in the bill of fare of the gouty. Among these may be particularly recommended lettuce, chiccory, artichokes, cucumbers, cardoons, salsify, celery, carrots, parsnips, and potatoes. Radishes, and especially the black radish, may be permitted, as well as the salads (lettuce, dandelion greens, water-cresses), but spinach and sorrel should be forbidden on account of the oxalic acid which they contain. As for cabbage and cauliflower, which contain much ni-

* Lecorché, "Theoretical and Practical Treatise on Gout." Paris, 1884.

trogen, one should be chary in their usage, as well as the starchy legumes,—beans, peas, and lentils.

As for bread, Bouchardat recommends, as far as possible, to replace this culinary product by potatoes, as is done in England; the importance of this I shall show you more fully when I come to speak of diet in diabetes. As regards fruits, they are all favorable, and the utility of strawberries and grapes has been especially insisted upon. The grape-cure is, in fact, sure to ameliorate the condition of many gouty patients.

As for mushrooms, truffles, and condiments, it is well to exercise great caution in their use.

In reference to drinks much may be said. As a general rule, the gouty person should drink abundantly. Water, which is the best drink, dissolves effete matters out of the tissues and flushes the kidneys, and is the best of diuretics. The alkaline waters are especially to be commended. Wine is not to be absolutely proscribed. What has led to the interdiction of spirituous and fermented liquors is the fact that when taken in excess they are among the most influential factors in the production of gout and gouty paroxysms, and Sydenham truthfully said that "if gluttony engenders gout, the latter is much oftener the effect of wine-drinking."

Wine must, then, be drank very sparingly, and, when taken, should be diluted with alkaline waters. The best wines for gouty people are the old wines that contain but little tannin, such as the light Bordeaux, or the mild pale wines; the sparkling (effervescent) wines, such as champagne, should be absolutely discarded. Scudamore, years ago, pointed out the disastrous effects of champagne on the gouty. The gaseous waters, such as Apollinaris, and the artificial seltzer-waters, should also be interdicted.

The strong beers, such as "stout," Bass's English ale, the Dublin porters, etc., should be proscribed. Garrod has shown us that in England the prolonged usage of these beers produces even in the poorer classes gouty manifestations. There has been much dispute as to the value of cider in gout and gravel. Some authorities (as Dumont) approve of it. Lecorché is, however, inclined to believe that in certain cases cider may be injurious.

In the case of cider, as of beer, the state of purity of the beverage has a good deal to do with the results experienced. As it is the custom at the present day to add alcohol to the beers of commerce, as well as to cider and perry, one can easily understand the effect of such falsifications in the diet of the gouty. If, therefore, clinicians claim to have seen good therapeutic effects from these fermented beverages, it is because pains were taken to procure an unsophisticated product. It need hardly be said that whiskey, strong wines, "bitters," cognac, etc., are to be absolutely proscribed.

As for tea and coffee, the latter may be taken in moderation, while tea should be forbidden, if for no other reason, because containing oxalic acid, as I shall shortly show you.

The hours for meals should be regulated with care, and the patient should eat slowly and masticate thoroughly. The meal should be plain, and Sydenham's observation on this point is good:

"I believe that the patient should content himself with one sort of food, for if he eats a variety, he is more likely to overload his stomach than if he satisfies his appetite with one kind. You can cure the gout if you can cure the gourmand of his gluttony."

Such are the dietetic rules applicable to gout. They are summed up in the following sentence from Sydenham, whose treatise on Gout (Podagra), published in 1683, still contains some of the best things that have ever been said on the subject of treatment: "The patient must observe great moderation in eating and drinking. On the one hand, he must eat no more food than the stomach can digest, and on the other hand, he must not enfeeble himself too much by abstinence; two extremes equally to be avoided, as I have many times proved on patients and on myself."

But the dietetic treatment of gout, if it be the most important, should, nevertheless, be supplemented by other hygienic measures, which I shall briefly indicate. The functions of the intestines, bladder, and skin must be carefully looked after. The podagrous patient must have a free stool every day, and if this cannot be brought about without cathartics, he should take every morning a glass of Pullna or Hunyadi Janos or Bouchardat's laxative, which consists of a drachm to half an ounce of Rochelle salts in a glass of lemonade.

The gouty patient should also regularly empty his bladder every two hours,—Bouchardat insists much on this necessity of complete evacuation of the bladder at frequent intervals,—and according to him, the untimely death of many illustrious men, and of Claude

Bernard in particular, was owing to the fact that they were habitually negligent in the performance of this important duty.

As for the care of the skin, the cutaneous functions should be maintained in a healthy state. This will be promoted by bathing the whole body and by massage every morning. Bouchardat adds once a week to the water of the bath a teaspoonful of essence of lavender, two of compound tincture of benzoin, and half an ounce of carbonate of potassium.

Exercise plays an important part in the hygiene of the gouty, equal, almost, to that of diet, since by exercise the victim of podagra is able to oxidize and burn his albuminoid substances. Exercise under all its forms, and proportioned to the endurance of the patient, should be prescribed; you know the sentence of our immortal fabulist,—

> "Goutte bien tracassée
> Est, dit-on, à demi pansée." *

But he that says gout, says gravel, and this leads me to finish this lecture by some remarks about urinary and hepatic lithiasis.

As for renal lithiasis, it is, as you know, comprehended under two great classes, acid gravel and alkaline gravel; the first includes uric and oxalic calculi, the second, lime deposits and the ammoniaco-magnesium phosphates.

As to the treatment of uric acid lithiasis, I have nothing to add to what I have said with reference to the diet of gout and the general regimé applicable thereto.

Oxalic acid gravel, called also "poor man's gravel," because it may be engendered by a diet exclusively vegetable, deserves to arrest our attention a few minutes.

There has been much dispute about oxaluria; some pretending with Bird, Prout, Garrod, Furhbringer, Ralfe, that oxalic acid is found in the normal state in the blood, constituting, when in excess, the oxalic diathesis; others, on the contrary, as Lecorché and Esbach, maintaining that oxalic acid when thus existing in the blood and excretions always comes from food. Without presuming to settle the dispute, I should be disposed from the stand-point of hygienic therapeutics to adopt the latter view, for if it is by no means proved that incomplete oxidation of saccharine and amylaceous substances may cause oxaluria, everybody is agreed in recognizing the preponderant rôle of vegetable aliments containing oxalic acid in determining this diathesis.

Hence, whenever you meet with cases of oxalic acid gravel, you ought rigorously to proscribe the usage of foods which contain this acid in relatively large quantities; as a guide you will find the following table from Esbach of use:

THE FOLLOWING SUBSTANCES CONTAIN IN EVERY 1000 PARTS OXALIC ACID IN THE ADJOINED PROPORTIONS:

1. *Groceries and Condiments.*

	Grammes.
Black tea, in substance	3.750
" infused five minutes	2.060
Cocoa	3.520 to 4.500
Chocolate	0.900
Pepper	3.250
Chiccory (infusion)	0.795
Coffee (adulterated)	0.127
Chervil	0.035
Parsley	0.006

2. *Farinaceous Substances.*

	Grammes.
White beans	0.312
Garden beans	0.158
Celery, radishes	0.135
Potatoes	0.046
Bread of good quality	0.047
Crust	0.130
Crumb	0.120
Buckwheat flour	0.171
Barley flour	0.039
Indian corn flour	0.033
Wheat bran	0.848

3. *Vegetables and Cooked Herbs.*

	Grammes.
Sorrel	2.740 to 3.630
Spinach	1.910 to 3.270
Garden rhubarb	2.466
Brussels cabbage	0.020
White cabbages	0.003
Beets	0.390
Green beans	0.060 to 0.212
Salsify	0.070
Tomatoes	0.002 to 0.052
Carrots	0.027
Celery stalks	0.025

4. *Salads.*

	Grammes.
Chiccory	0.103
Endives	0.045
Escarols	0.017
Corn salad	0.016

5. *Fruits.*

	Grammes.
Dried figs	0.270
Currants	0.130
Plums	0.120
Gooseberries	0.070
Prunes	0.070

* *I.e.*, Exercise is the best dressing.

	Grammes.
Raspberries	0.062
Oranges	0.030
Lemons	0.030
Cherries	0.025
Strawberries	0.012

I must call your attention to certain points connected with the above table. It will be seen that tea contains almost as much oxalic acid as sorrel. There is also a notable proportion in coffee, while in this respect cocoa stands almost first. Hence chocolate, tea, and coffee should be forbidden in oxaluria.

You also notice the large proportion of oxalic acid in bran, hence it is that coarse bread favors the development of oxalic gravel.

As for vegetables, the table shows that it is a mistake to forbid the tomato, while allowing spinach. The latter contains per kilogramme 1.910 to 3.270 of oxalic acid, while tomatoes contain only 0.002 to 0.005.

As for ammoniacal deposits, they are always secondary, and the result of fermentation of the urine, brought about by affections of the vesical mucosa. Here, as Guyon has shown, it is the milk diet which gives the best results, and in suppurative pyelitis and suppurative cystitis you should put your patient on a regimen exclusively of milk.

Hepatic calculi have a quite different origin. They are due to the presence of cholesterine in the biliary passages, and demand a special hygienic treatment. These calculi are constituted almost exclusively of cholesterine and bile pigment.

Cholesterine is, as you know, a non-saponifiable fat, which Berthelot's labors have placed among the alcohols. Under certain conditions, which I shall point out, cholesterine is deposited in the bile-passages. Knowing these conditions, you will be able to direct aright the hygienic treatment of biliary lithiasis.

Two circumstances predispose to the precipitation of cholesterine,—either it is in excess in the bile, or, while existing in only the normal proportion, its precipitation is brought about by modifications in the composition of the bile. Excessive production of cholesterine is generally due to one of two causes,—either to a diet too rich in fats, or to overtaxing the nervous system. Flint, as you are aware, considers cholesterine as a product of disassimilation of the nervous system.

With regard to precipitation by modifications in the bile, Thenard (who has well studied this subject) has shown that cholesterine, while remaining at the normal figure in the bile, may, nevertheless, be deposited when the bile loses its alkalinity, which is most likely to happen under a diet of animal food. Do not forget also that stagnation of bile in the gall-bladder favors this deposition. The dietetic rules applicable to biliary lithiasis easily suggest themselves from the considerations just stated.

You should suppress from the habitual dietary all fatty substances, carbo-hydrates, sugars, and starches which may engender cholesterine. Among starchy legumes, peas ought especially to be discarded, because containing a fatty body similar to cholesterine, as Hœckel and Schlagdenhauffen have shown, and which they have named *vegetable cholesterine*.*

While on this subject of vegetable cholesterine, I must mention, only to condemn it, a popular notion that carrots constitute an excellent food in hepatic affections, and particularly in biliary lithiasis. This notion seems to be a survival of an idea, based on the old doctrine of similars, called also the doctrine of signatures, whereby a yellow vegetable, like the carrot, was regarded as a specific for jaundice. On the contrary, the composition of the carrot shows it to contain cholesterine, and hence to be unfit for food in biliary lithiasis, as may be seen by referring to Arnaud's analysis.†

Besides the sugar which it contains, the carrot also possesses a hydrocarbon called *carottine*, having for formula $C_{40}H_{56}$, and *hydro-carottine*, which is really a vegetal cholesterine. Its main ingredients are, in fact, sugar, carbo-hydrates, and cholesterine, substances which render this vegetable inappropriate for the diet of persons affected with gall-stone.

You will also avoid the too exclusive use of animal food. Lastly, you will urge the moderate usage of eggs, and if you were to sum up the bill of fare of a person predisposed to gall-stones, you would say, never more than one egg a day, a mixed diet of meat and fresh vegetables. All kinds of meat are permissible, but the patient should discard the fatty parts. All kinds of fresh vegetables are proper, and cannot but be

* Hœckel et Schlagdenhauffen, "Sur la présence de la cholestérine dan quelques corps gras d'origine végétale" (*Acad. des Sc.*, 1886, t. ciii. p. 1317).

† Arnaud, "Recherches sur la composition de la carottine, sa fonction chimique et sa formule; Sur la présence de la cholestérine dans la carotte, recherches sur ce principe immédiat" (*Acad. des Sc.*, 1886, t. cii. p. 1119 et 1319).

beneficial. While starchy foods in general are to be avoided, potatoes may be allowed. Bread should be eaten in great moderation.

Acid fruits are also to be recommended, and Bouchardat* speaks highly of their utility, but sweet fruits are to be avoided, and pastries should be absolutely forbidden. The meals should be near enough together so that the gall-bladder may be frequently emptied. As for drinks, you may allow wine well diluted with some alkaline water, as Vichy or Vals. Free action of the bowels should be promoted, and the bodily exercise should be sufficient to keep the general functions of the economy in a high state of efficiency. Such are the hygienic rules applicable to biliary lithiasis.

In the next lecture I shall consider a subject of the greatest interest, namely, diet in diabetes.

A CONTRIBUTION TO OUR KNOWLEDGE OF FEVER, AND THE AGENTS WHICH PRODUCE OR ARREST IT.

By Drs. H. C. Wood, E. T. Reichert, and Hobart A. Hare.

(Concluded from page 741.)

THE second drug whose action upon the thermogenic functions of the body we have studied is antipyrin. Clinical experience has certainly demonstrated that the influence of this agent upon temperature in febrile diseases is far greater and more constant than is that of quinine; in correspondence with this, we have found that it is much more apt to decrease the temperature of the normal dog than is quinine. At one time in the course of our experiments we thought that this temperature depressing influence was constant. A larger experience has shown that, though very frequently full doses of antipyrin do depress the temperature of the normal animal, in other cases no such effect is produced. If the antipyrin be given in overdoses it produces violent tetanic convulsions, which are accompanied by a very marked rise of the temperature, amounting sometimes to several degrees. This rise of temperature seems to us to be caused, at least in part, by the convulsions, and may not at all be due to any immediate action of the drug. It is very rapidly developed after the coming on of the convulsion. The convulsions are of cerebral origin, although they had the appearance of being spinal, since, after section of the cord, they failed to appear in the hind legs, although present in the front legs.

In our investigation of antipyrin our first efforts were directed to determining whether it has any constant influence upon the dissipation or production of animal heat in health. In these experiments the antipyrin was given in doses of one-half grain, or of one grain per pound, to the animal; and in all cases the dogs were in as nearly perfect health as we could obtain.

ANTIPYRIN.

Experiment 57.—Dog; weight, 19 pounds.

Time.	Rec. temp.	Box temp.
12.55	103.7°	68.90°
1.55	.....	69.70°
2.55	103.7°	70.45°
		1.55°

3. 10 grains of antipyrin.

Time.	Rec. temp.	Box temp.
3.15	103.7°	70.15°
4.15	.....	70.68°
5.15	103.3°	71.20°
	0.4°	1.05°

RESULTS.

Hourly dissipation of heat	94.55
Hourly production of heat	94.55
Hourly dissipation of heat	64.050
Hourly production of heat	61.200

SUMMARY.

Dissipation of heat before antipyrin	94.55
Dissipation of heat after antipyrin	64.05
Loss of heat dissipation after antipyrin	30.50
Production of heat before antipyrin	94.55
Production of heat after antipyrin	61.2
Loss of heat production after antipyrin	33.3

Experiment 58.—Dog; weight, 20 pounds.

Time.	Rec. temp.	Box temp.
12.05	103.3°	61.70°
1.05	103.5°	62.65°
	0.2°	0.95°

1.15 20 grains of antipyrin.

Time.	Rec. temp.	Box temp.
2.15	103.5°	63.95°
3.15	103.5°	64.80°
		0.85°

RESULTS.

Hourly dissipation of bodily heat	115.90
Hourly production of heat	118.90
Hourly dissipation of heat	103.70
Hourly production of heat	103.7

SUMMARY.

Dissipation of heat before antipyrin	115.90
Dissipation of heat after antipyrin	103.70
Loss of heat dissipation after antipyrin	12.20

* Bouchardat, "On the Hygienic Treatment of Hepatic Calculi" (*Bull. de Thér.*, t. xcix. p. 145).

Production of heat before antipyrin..... 118.90
Production of heat after antipyrin..... 103.7

Loss of heat production after antipyrin.. 15.2

Experiment 59.—Dog; weight, 20 pounds, 8 ounces.

Time.	Rec. temp.	Box temp.
11.55	103°	61.40°
12.55	.....	62°
1.55	103°	62.70°
		1.30°

2. 10 grains of antipyrin.

Time.	Rec. temp.	Box temp.
2.05	103°	62.60°
3.05	.....	63.20°
4.05	103°	63.70°
		1.10°

RESULTS.

Hourly dissipation of heat..... 79.30
Hourly production of heat.....
Hourly dissipation of heat..... 67.10
Hourly production of heat.....

SUMMARY.

Dissipation of heat before antipyrin..... 79.30
Dissipation of heat after antipyrin..... 67.10

Loss of heat dissipation following antipyrin. 32.2

Production of heat before antipyrin..... 79.30
Production of heat after antipyrin..... 67.10

Loss of heat production after antipyrin.... 32.20

Experiment 60.—Dog; weight, 21 pounds.

ANTIPYRIN.

Time.	Rec. temp.	Box temp.
10.05	104.90°	52.50°
11.05	.....	54.20°
12.05	104.35°	55.60°
	— 0.55°	3.10°

12.10 20 grains of antipyrin.

Time.	Rec. temp.	Box temp.
12.15	104.35°	55.90°
1.15	.....	57.30°
2.15	104.50°	58.65°
	+ 0.15°	2.75°

RESULTS.

Hourly dissipation of heat..... 189.1000
Hourly production of heat..... 184.7688
Hourly dissipation of heat..... 167.7500
Hourly production of heat..... 168.9313

SUMMARY.

Dissipation of heat before antipyrin..... 189.1
Dissipation of heat after antipyrin..... 167.75

Loss of heat after antipyrin..... 21.35

Production of heat before antipyrin..... 184.7688
Production of heat after antipyrin..... 168.9313

Loss of heat production after antipyrin. 15.8375

Experiment 61.—Dog; weight, 21.50 pounds.

Time.	Rec. temp.	Box temp.
10.50	103.5°	60°
11.50	.....	60.80°
12.50	104.2°	62.20°
	+ 0.7°	2.20°

12.55 21 grains of antipyrin.

Time.	Rec. temp.	Box temp.
1.	104.2°	62.20°
2.	.....	63.10°
3.	103.4°	64.35°
	— 0.8°	2.15°

RESULTS.

Hourly dissipation of heat..... 134.2000
Hourly production of heat..... 139.8437
Hourly dissipation of heat..... 131.1500
Hourly production of heat..... 124.7000

SUMMARY.

Dissipation of heat before antipyrin..... 134.2
Dissipation of heat after antipyrin..... 131.15

Loss of heat dissipation after antipyrin. 3.05

Production of heat before antipyrin..... 139.8437
Production of heat after antipyrin..... 124.7

Loss of heat production after antipyrin. 15.1437

Experiment 62.—Dog; weight, 9 pounds. Small box.

ANTIPYRIN.

Time.	Rec. temp.	Box temp.
1.05	104.2°	68.2°
2.05	103°	69.7°
	— 1.2°	1.5°

2.15 9 grains of antipyrin hypodermically.

Time.	Rec. temp.	Box temp.
2.16	103.2°	70.4°
3.16	102.1°	71°
	— 1.1°	0.6°

RESULTS.

Hourly dissipation of heat..... 113.355
Hourly production of heat..... 105.255
Hourly dissipation of heat..... 45.342
Hourly production of heat..... 37.917

SUMMARY.

Dissipation of heat before antipyrin..... 113.355
Dissipation of heat after antipyrin..... 45.34

Loss of heat dissipation after antipyrin.. 68.015

Production of heat before antipyrin..... 105.25
Production of heat after antipyrin..... 37.9

Loss of heat production..... 67.35

Experiment 63.—Dog; weight, 5 pounds.

ANTIPYRIN.

Time.	Rec. temp.	Box temp.
11.30	102.6°	71.00°
12.30	102.5°	71.65°
	0.1°	0.65°

12.35 5 grains of antipyrin hypodermically.

Time.	Rec. temp.	Box temp.
12.40	102.5°	71.65°
1.40	101.1°	72.50°
	1.4°	0.85°

RESULTS.

Hourly dissipation of bodily heat	49.1205
Hourly production of heat	48.7455
Hourly dissipation of bodily heat	64.2345
Hourly production of heat	54.7345

SUMMARY.

Heat dissipation before antipyrin	49.1205
Heat dissipation after antipyrin	64.2345
Gain of heat dissipation after antipyrin	15.1140

Experiment 64.—Dog ; weight, 6 pounds.

Time.	Rec. temp.	Box temp.
1.50	103.1°	72.10°
3.20	104.0°	72.45°
	0.9°	0.35°

3.30 6 grains of antipyrin.

Time.	Rec. temp.	Box temp.
3.35	104°	72.40°
5.05	102.9°	73.30°
	1.1°	0.90°

RESULTS.

Hourly dissipation of heat	17.6330
Hourly production of heat	20.3330
Hourly dissipation of heat	45.342
Hourly production of heat	43.042

SUMMARY.

Dissipation of heat before antipyrin	17.6330
Dissipation of heat after antipyrin	45.3420
Gain	27.7090
Production of heat before antipyrin	20.330
Production of heat after antipyrin	43.042
Gain	22.712

Experiment 65.—Dog ; weight, 9 pounds.

ANTIPYRIN.

Time.	Rec. temp.	Box temp.
10.45	104.4°	66.20°
11.45	.....	67.15°
12.45	102.3°	67.95°
	2.1°	1.75°

12.55 9 grains of antipyrin.

Time.	Rec. temp.	Box temp.
1.20	102.5°	67.75°
2.20	.....	.....
3.20	101.5°	69.20°
	1.0°	1.45°

RESULTS.

Hourly dissipation of heat	66.1237
Hourly production of heat	59.1367
Hourly dissipation of heat	54.7882
Hourly production of heat	50.9182

SUMMARY.

Dissipation of heat before antipyrin	66.1237
Dissipation of heat after antipyrin	54.7882
Loss of heat dissipation after antipyrin	11.3355
Production of heat before antipyrin	59.1367
Production of heat after antipyrin	50.9182
Loss of heat production after antipyrin	8.2285

Antipyrin Experiments on Normal Dogs. Table No. 1.

Experiment.	Duration.	Weight of dog.	Dose.	Hourly heat dissipation.				Hourly heat production.			
				Normal.	After drug.	Decrease.	Percentage.	Normal.	After drug.	Decrease.	Percentage.
Number	Hours.	Pounds.	Grains.								
57	4	19	10	94.55	64.05	30.500	32	94.550	61.200	33.350	35
58	2	20	20	115.900	103.70	12.200	10.5	118.90	103.70	15.200	12.5
59	4	20.5	10	79.300	67.10	32.20	40.5	79.30	67.10	32.200	40.5
60	4	21	20	189.100	167.750	21.350	11	184.7688	168.9313	15.8375	8.5
61	4	21.50	21	134.200	131.150	130.500	2	139.8437	124.700	15.1437	10.5
62	2	9	9	113.355	45.345	68.015	60	105.255	37.917	67.3580	64
65	4	9	9	66.1237	54.7882	11.355	17	59.1367	50.9182	8.2285	13.5

Antipyrin Experiments on Normal Dogs. Table No. 2.

Experiment.	Duration.	Weight of dog.	Dose.	Hourly heat dissipation.				Hourly heat production.			
				Normal.	After drug.	Increase.	Percentage.	Normal.	After drug.	Increase.	Percentage.
Number	Hours.	Pounds.	Grains.								
63	2	5	5	49.1205	64.2345	15.114	23	48.7455	54.7345	5.9890	12
64	2	6	6	17.633	45.342	27.7090	150	20.330	43.0420	22.712	111

The records of these experiments and the tables in which the results obtained are summarized show that in seven of the experiments there was a decrease in the production and dissipation of animal heat, whilst in two instances both of these functions were distinctly increased. The results obtained are therefore not entirely concordant. The explanation of the two exceptional experiments may, perhaps, be found in the fact that they were made upon very small, feeble dogs, to which was given the largest dose of the antipyrin employed. When administered in very large doses, we have found that antipyrin increases the animal temperature very greatly,—whether by direct or an indirect action is not known. Such increase we have only seen when convulsions have been present, and we are therefore inclined to attribute the rise of temperature directly to the convulsion; but it is possible that the drug may cause increased thermogenesis independently of the convulsive movement. Moreover, it is not at all unlikely that in the exceptional experiments the dogs had a convulsion in the calorimeter, for, unless such convulsion were exceedingly violent and prolonged, it might very well pass unnoticed.

Further, the reasons given during the discussion of quinine—why in calorimetrical experiments with drugs absolutely concordant results are hardly to be expected—are here also applicable.

Whatever may be the correct explanation of the results reached in the exceptional experiments, the whole series show that, in the normal dogs, antipyrin, given in doses of ½ to 1 grain per pound, usually produces a distinct decrease both of heat production and of heat dissipation. In only one of the seven experiments was the decrease in heat dissipation greater than the decrease in heat production. In one experiment the two functions were equally affected; in five experiments heat production was distinctly increased more than was heat dissipation. These facts prove that, if the change in either function is the cause of the change in the other function, it must be heat production which is dominant,—*i.e.*, is the function primarily affected and affecting the other function. In other words, we must conclude that heat production in normal dogs is in some way primarily lessened by antipyrin, and that such lessening is followed either as an accompaniment or effect by lessening of the heat dissipation.

The question now naturally presents itself, In what way does antipyrin lessen heat production? But before entering upon the discussion of this question it seems wisest to consider the action of antipyrin upon the temperature of dogs in which fever has been produced by the pepsin-peptone. Our experiments upon the subject have all been made on the plan adopted with quinine.

Experiment 66.—Dog; weight, 20 pounds.

PART I.

Time.	Rec. temp.	Box temp.
11.45	102.9°	58.45°
12.45	.....	59.35°
1.45	103°	60.15°
	0.1°	1.70°

2. Injected 12 grms. boiled pepsin into jugular vein.
2.15 Injection ended.

Time.	Rec. temp.	Box temp.
3.20	103°	60.30°
	104.6°	61.05°
	1.6°	0.75°

Time.	Rec. temp.	Box temp.
3.40	104.6°	61.20°
4.40	.....	62.40°
5.40	105°	63.35°
	0.4°	2.15°

RESULTS.

Hourly dissipation of heat before pepsin..... 103.700
Hourly production of heat..... 104.450
Hourly dissipation of heat after pepsin..... 91.500
Hourly production of heat..... 115.500
Hourly dissipation of heat after pepsin..... 131.150
Hourly production of heat after pepsin..... 134.150

Experiment 66.—

PART II.

Time.	Rec. temp.	Box temp.
11.45	104°	65.85°
12.45	.....	66.80°
1.45	103.2°	67.45°
	0.8°	1.60°

1.50 Gave 12 grms. of boiled pepsin in jugular.
2.00 Gave 20 grs. of antipyrin into jugular.

Time.	Rec. temp.	Box temp.
2.30	104.8°	67.50°
2.30	104°	68.50°
	0.8°	1°

Time.	Rec. temp.	Box temp.
3.35	104°	68.50°
4.35	.....	69.25°
5.35	102.7°	69.90°
	1.3°	1.40°

RESULTS.

Hourly dissipation of heat before drugs 97.600
Hourly production of heat before drugs... ..... 91.600
Hourly dissipation of heat after drugs..... 122.000
Hourly production of heat after drugs..... 110.000
Hourly dissipation of heat after drugs..... 85.400
Hourly production of heat after drugs.. ..... 75.650

Experiment 67.—Dog; weight, 17 pounds.

PART I.

Time.	Rec. temp.	Box temp.
10.35	105.5°	62.55°
11.35	.....	62.85°
12.35	105.8°	63.45°
	0.3°	0.90°

1. Injected 12 grammes pepsin, boiled, into jugular.
1.05 Injection.

Time.	Rec. temp.	Box temp.
1.20	105.3°	63.60°
2.20	.....	.....
3.20	.....	64.50°
4.20	106.1°	65°
	0.8°	1.40°

RESULTS.

Hourly dissipation of heat before pepsin..... 54.900
Hourly gain of heat before pepsin..... 1.912
Hourly production of heat before pepsin. 56.812
Hourly dissipation of heat after pepsin..... 85.400
Hourly gain of heat after pepsin..... 5.100
Hourly production of heat after pepsin... 90.500

Experiment 67.—Dog; weight, 17 pounds.

PART II.

Time.	Rec. temp.	Box temp.
12.30	103.5°	59.45°
1.30	.....	59.65°
2.30	103.5°	.....
		0.20°

2.40 Injected 12 grammes pepsin, boiled, into jugular.
2.45 Injection ended.
2.46 Injected 17 grains antipyrin into jugular.
2.50 Injection ended.

Time.	Rec. temp.	Box temp.
3.15	102.3°	59.35°
4.15	.....	.....
5.15	.....	.....
6.15	103°	60.40°
	0.7°	1.05°

RESULTS.

Hourly dissipation of heat before drugs..... ..... 24.400
Hourly production of heat before drugs..... 24.400
Hourly dissipation of heat after drugs..... 42.700
Hourly production of heat after drugs..... 44.975

Experiment 68.—*Fever and Antipyrin.*—Dog; weight, 22 pounds.

PART I.

Time.	Rec. temp.	Box temp.	Temp. air.
9.20	102.5°	54.50°	52.50°
10.20	.....	.....	.....
11.20	104°	56.20°	65.50°
	1.5°	1.70°	

11.25 Injected 12 grammes pepsin, boiled, into jugular.
11.30 Injection ended.

Time.	Rec. temp.	Box temp.	Temp. air.
11.40	103.3°	55.50°	64°
12.40	.....	57.10°	61.50°
1.40	105.4°	57.75°	61°
	2.1°	2.25°	

RESULTS.

Hourly dissipation of heat before pepsin..... 103.700
Hourly production of heat before pepsin..... 116.075
Hourly dissipation of heat after pepsin..... 137.250
Hourly production of heat after pepsin..... 154.575

Experiment 68.—*Fever and Antipyrin.*—Dog; weight, 22 pounds.

PART II.

Time.	Rec. temp.	Box temp.	Temp. air.
10.35	104.5°	57.45°	64°
11.35	.....	58.10°	66°
12.35	104.5°	58.80°	68°
		1.35°	

12.55 Injected 12 grammes of boiled pepsin and 22 grains of antipyrin into jugular.
1. Injection ended.

Time.	Rec. temp.	Box temp.	Temp. air.
1.20	102.8°	58.90°	66.50°
2.20	.....	59.85°	65°
3.20	104°	60.65°	75°
	1.2°	1.75°	

RESULTS.

Hourly dissipation of heat before drugs 82.350
Hourly production of heat before drugs..... 82.350
Hourly dissipation of heat after drugs..... 106.750
Hourly production of heat after drugs..... 116.650

We have tabulated the results of these experiments in two sets of tables similar to those which we made concerning quinine, and, as these tables were at that place thoroughly explained, we shall not here give any discussion of the method of their construction.

Fever and Antipyrin Experiments. Heat Dissipation.

	Without Antipyrin. Heat dissipation on first day.			With Antipyrin. Heat dissipation on second day.		
Exp.	Before pepsin.	First period after pepsin.	Second period after pepsin.	Before pepsin.	First period after pepsin.	Second period after pepsin.
66	R. T. 102.9°–103° D. 103.700	R. T. 103°–104.6° D. 91.500	R. T. 104.6°–105° D. 131.150	R. T. 104°–103.2° D. 97.600	R. T. 104.8°–104° D. 122.000	R. T. 104°–102.7° D. 85.400
67	R. T. 105.5°–105.8° D. 54.900	R. T. 105.3°–106.1°* D. 85.400		R. T. 103.5°–103.5° D. 24.400	R. T. 102.3°–103° D. 42.700	
68	R. T. 102.5°–104° D. 103.700	R. T. 103.3°–105.4° D. 137.250†	R. T. D.	R. T. 104.5°–104.5° D. 82.350	R. T. 102.8°–104° D. 106.750	R. T. D.

* Dog in calorimeter for three hours without interruption. † Dog in calorimeter for two hours only without interruption.

Fever and Antipyrin Experiments. Heat Production.

	Without Antipyrin. Heat production on first day.			With Antipyrin. Heat production on second day.		
Exp.	Before pepsin.	First period after pepsin.	Second period after pepsin.	Before pepsin.	First period after pepsin.	Second period after pepsin.
66	R. T. 102.9°–103° P. 104.450	R. T. 103°–104.6° P. 115.500	R. T. 104.6°–105° P. 134.150	R. T. 104°–103.2° P. 91.600	R. T. 104.8°–104° P. 110.000	R. T. 104°–102.7° P. 75.650
67	R. T. 105.5°–105.8° P. 56.812	R. T. 105.3°–106.1°* P. 90.500		R. T. 103.5°–103.5° P. 24.400	R. T. 102.3°–103° P. 44.975	
68	R. T. 102.5–104° P. 116.075	R. T. 103.3°–105.4° P. 154.575†	R. T. P.	R. T. 104.5°–104.5° P. 82.350	R. T. 102.8°–104° P. 116.650	R. T. P.

* Dog in calorimeter three hours without interruption. † Dog in calorimeter two hours only.

Tables of Percentages in Fever and Antipyrin Experiments. Heat Dissipation.

Increase marked +. Decrease marked —.

	First day, without antipyrin.				Second day, with antipyrin.			
Experiment.	First period after pepsin.		Second period after pepsin.		First period after pepsin and antipyrin.		Second period after pepsin and antipyrin.	
	Absolute.	Percentage.	Absolute.	Percentage.	Absolute.	Percentage.	Absolute.	Percentage.
66	— 12.200	— 11	+ 27.450	+ 30	+ 24.400	+ 20	+ 12.200	— 30
67*	+ 30.500	+ 35	+ 30.500	+ 35	+ 18.300	+ 76	+ 18.300	+ 76
68†	+ 33.550	+ 25	+ 33.550	+ 25	+ 24.400	+ 22	+ 24.400	+ 22

* Dog was in the calorimeter for both periods (equalled three hours) *without interruption.* For this reason the figures are duplicated in order to come properly into the table.
† Same as above, only the periods equalled two hours instead of three hours.

Tables of Percentages of Fever and Antipyrin Experiments. Heat Production.

Increase marked +. Decrease marked —.

Experiment.	First day, without antipyrin.				Second day, with antipyrin.			
	First period after pepsin.		Second period after pepsin.		First period after pepsin and antipyrin.		Second period after pepsin and antipyrin.	
	Absolute.	Percentage.	Absolute.	Percentage.	Absolute.	Percentage.	Absolute.	Percentage.
66	+ 11.050	+ 9½	+ 29.700	+ 13½	+ 18.400	+ 16½	— 15.950	— 31
67*	+ 33.688	+ 37	+ 33.688	+ 37	+ 20.575	+ 84	+ 20.575	+ 84
68†	+ 38.500	+ 25	+ 38.500	+ 25	+ 34.300	+ 30	+ 34.300	+ 30

* Dog was in calorimeter for both periods (equalled three hours) without interruption. For this reason the figures are duplicated in order to come properly into the table.
† Same as above, only the periods equalled two hours instead of three hours.

An examination of the table of heat dissipation will show that in Experiment 66 there was a much greater dissipation of heat the first hour after the pepsin and antipyrin than in the first hour immediately after the pepsin alone, but if the whole time of Experiment 66 be considered, the increase in the heat dissipation after the pepsin and antipyrin was very distinctly less than after the pepsin alone. This experiment is, therefore, in exact accord with the results reached in 67 and 68, for, it must be remembered, in Experiments 67 and 68 the whole time after the injection of the drugs was studied at one calorimetrical run.

Considering next the table of heat production, we find that the readings are parallel with those of the table of heat dissipation. Taking each of the three experiments as a whole, there is a very decidedly lessened increase of heat production after the injection of the antipyrin and pepsin as compared with the increase after the pepsin alone. On the other hand, there was an augmented increase of heat production for the first hour after the injection of the pepsin and antipyrin in Experiment 66 as compared with the first hour after the pepsin alone on the previous day.

This is exactly as in the heat dissipation. The conclusion must be drawn that antipyrin diminishes in pepsin-peptone fever both heat dissipation and heat production.

When we come to study the question as to whether the heat dissipation or heat production was most profoundly diminished, we find that in Experiment 66 during the three hours in which the animal was in the calorimeter the first day after the pepsin, in round numbers 42 more units of heat were dissipated than ought to have been if the animal had remained without fever. Thus $(27 \times 2) - 12 = 42$, whilst, on the second day after the pepsin and antipyrin, there was no increase in heat dissipation, $24\ (12 \times 2) - 0$. In regard to heat production in the same experiment, there was, on the first day after the pepsin alone an absolute increase of 69 units $(29 \times 2) + 11 = 69$; but on the second day there was an absolute lessening of 12 units in the heat production, $18 - 30 = 12$. Experiment 66 may very well be expressed in the following table:

	First day.	Second day.	Difference.
Heat dissipation.....	42	0	42
Heat production	69	— 12	81

It is perfectly plain that heat production was affected in Experiment 67 much more than was heat dissipation.

In Experiment 67 heat dissipation on the first day after the pepsin alone was for the three hours during which the dog was in the calorimeter increased 90 units. For the corresponding period in the second day after the pepsin and antipyrin the increase was 54 units. Heat production was increased the first day 99 units, the second day 60 units.

	First day.	Second day.	Difference.
Heat dissipation.....	90	54	36
Heat production.....	99	60	39

In Experiment 67, therefore, antipyrin affected heat production more than it did heat dissipation.

Computing Experiment 68, and stating the results in tabular form, as in the other similar experiments, we find,—

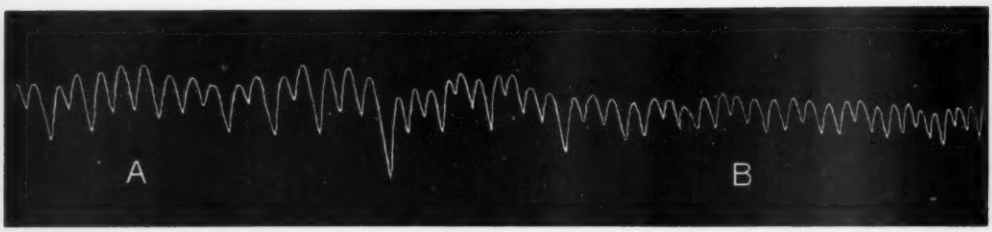
	First day.	Second day.	Difference.
Heat dissipation.....	66	48	18
Heat production.....	76	68	8

In Experiment 68 heat dissipation was more profoundly affected than was heat production.

So far as relates to the relative action of antipyrin in the two functions of heat dissipation and heat production, our experiments do not yield accordant results. Twice heat production was more affected than was heat dissipation; once dissipation was the more influenced. The most probable conclusion that can be drawn from all our experiments is that antipyrin affects more pronouncedly, both in health and in the fevered dogs, heat production than heat dissipation, but sometimes dissipation seems to be the more influenced. It is possible that if Experiment 68 had been run during the whole period of the fever instead of the two hours the results would have been in accord with those of 66 and 67, for it was precisely in the latter stages of the fever that the action of the antipyrin seems most pronounced.

The next question which presented itself for solution was, Is this action of antipyrin upon the animal temperature due to an influence exerted upon the circulation, or is its effect produced in any other way? In order to determine this we have made a series of experiments upon the effect of antipyrin on the temperature of the dog suffering from fever. The records of these experiments are as follows:

EXPERIMENT 69.



Tracing showing effect of the injection of 10 grains of antipyrin at 3.05 P.M. Injection begun at A and ended at B.

Experiment 69.—Antipyrin alone; weight of dog, 24 pounds. No pepsin given. Effects of antipyrin on normal temperature and blood-pressure.

Time.	Drug.	Press.	Temp.	Remarks.
2.35	.....	194	104.5°	
2.40	.....	170–194	.....	
2.45	5 grs.	180	.....	
2.46	.....	170–190	.....	
2.47	.....	170–190	.....	
3.05	10 grs.	168–170	.....	
3.06	.....	.....	.....	Lost blood.
3.07	.....	186–188	.....	
3.19.50	.....	160–162	.....	
3.20	10 grs.	160–164	.....	
3.22.50	.....	160	.....	
3.23	15 grs.	160	.....	
3.25	.....	180–190	.....	Violent breathing.
3.27	.....	180–196	.....	
3.30	.....	178–180	.....	
3.50	.....	160–170	103.5°	Stopped breathing for 1 or 2 minutes, then began to breathe hurriedly and slightly.
4.	.....	190–192	102.5°	
4.15	.....	160	102.5°	Breathing in paroxysms for 10 to 20 seconds, then breathing fast and strong for 3 to 10 seconds.
4.30	.....	160–164	102.1°	
4.45	.....	170–180	101.7°	
5.15	.....	164–170	.....	Breathing a little improved; struggling.
5.20	.....	180–200	.....	
5.25	.....	170–180	101.5°	Dog when untied strong and lively.

Experiment 70.—Bull-dog; weight, 30 pounds. Effects of antipyrin on normal temperature and pressure.

Time. P.M.	Drug.	Press.	Pulse.	Temp.	Remarks.
2.50	.....	180–190	150	105.3°	Has been struggling.
2.53	25 grs.	184–190	150	.....	
2.53.02	.....	220–244	not countable.	.....	Tracing irregular dog struggling.
2.55	.....	210–220	120	.....	Straight lines owing to struggles or asphyxia.
2.55.10	20 grs.	195–220	110–120	.....	Struggling.
3.05	.....	200–210	180	104.5°	
3.06	15 grs.	196–200	180	.....	
3.06.02	.....	.....	.....	.....	Struggling.
3.20	.....	200	200		
3.25	30 grs.	180–200	180		
3.25.02	.....	200	120	.....	Struggling.
3.45	.....	.....	.....	105°	
3.50	30 grs.	.....	.....	.....	Violent tetanic convulsions.
4.	.....	.....	.....	107.3°	Tetanus.
4. P.M.					

Experiment 71.—*Pepsin and Antipyrin.*—Cur; weight, 30 pounds. Pepsin, boiled filtrate, 20 cc.–4 grms. of pepsin.

Time.	Drug.	Press.	Pulse.	Temp.	Remarks.
11.15	.....	196–200	84–86	102.2°	
11.16	4 grms. pepsin.				
12.08	4 grms. pepsin.	160–174	.....	103.5°	
12.17	4 grms. pepsin.	160	150		
12.19	3 grms. pepsin.	160–164	180		

Time.	Drug.	Press.	Pulse.	Temp.	Remarks.
12.20	.....	.....	.....	102.8°	
1.	.....	clots.	.....	clots.	
1.05	.....	140–150	.....	102.5°	
1.30	.....	150	.....	104°	
2.	.....	160	186	104.4°	
2.30	.....	150–160	132	105.5°	
3.	.....	170–190	120	105.5°	
3.30	.....	140–160	138	105.7°	
3.40?	antipyrin, 20 or 30 grs.	200			See tracing. Amount of antipyrin stated is contradictory here.
4.45	.....	200	132	106.7°	Breathing rapid; convulsion.
4.55	.....	.....	.....	107.7°	Dog quiet.
5.40	.....	150–160	138	102.3°	Quiet.
5.41	.....	.....	.....	.....	Killed dog.

Dog struggling violently ever since injections. Never quiet more than a minute. Howling.

Of these experiments, the first (Experiment 69) shows that in the normal dog the fall of bodily temperature produced by antipyrin is not the result of any general action upon the circulation, since the arterial pressure stood at from 170 to 190 at a time when the rectal temperature was 104.5° F., and from 170 to 180 after the rectal temperature had fallen 3° F.

The second experiment (Experiment 70) is interesting as showing that, when the violent convulsions produced by antipyrin are fully developed, the rectal temperature rises very decidedly, which rise is probably produced by the convulsion and not by any direct action of the antipyrin.

The third experiment (Experiment 71) proves that the fever produced by pepsin filtrate is controlled by antipyrin without the circulation being notably affected. Under the influence of the pepsin the rectal temperature was put up from 102.2° to 107.7°, the arterial pressure falling from 196 to 170, and then, after the administration of antipyrin, the temperature fell in three-quarters of an hour 5.4°F., with only a very slight further reduction of the arterial pressure. The mere withdrawal of the heat would, we think, be enough to produce this fall of arterial pressure.

In an earlier portion of the present memoir we demonstrated that there is no necessary connection between the arterial pressure and the bodily temperature. In accord with this, the experiments that we have made upon the bodily temperature and blood-pressure under the influence of antipyrin prove that antipyrin diminishes the bodily temperature without affecting the arterial pressure. Our previous experiments have shown that the fall of the bodily temperature produced by antipyrin is the result of a lessened production of animal heat. It follows from all this that antipyrin lessens the production of animal heat without affecting the arterial pressure. As it is hardly possible that there could be any effect upon the circulation sufficiently powerful to markedly influence thermogenesis without such effect being registered in the general blood-pressure, we are forced to conclude that antipyrin in some direct way influences the chemical alterations of the body.

Up to this point we believe that the matter has been one of experimental determination, but beyond it we are at present forced to theorize simply. Antipyrin may stimulate the inhibitory heat-centre of Setschenow, and thereby check the chemical movements of the organism, or it may have some direct relationship with the various protoplasmic masses of the system, and by an immediate action upon them lessen their chemical activities. The first of these theories or suppositions seems to us most probable, although we cannot at present offer proof of its correctness. In accordance with it the rise of bodily temperature (and apparently of heat production) which we have seen follow the exhibition of overwhelming doses of antipyrin is readily explained as the result of a paralysis of Setschenow's centre. As is universally acknowledged, many nervous stimulants become in overdoses paralyzants; and in accordance with the theory which we at present favor, antipyrin in moderate dose acts as a stimulant to the inhibitory heat-centre, lessens the production of animal heat, and tends to decrease the bodily temperature; but it is possible that when in large quantity it paralyzes the inhibitory heat-centre, and thereby removes the natural check of the chemical movements of the body, which removal is necessarily followed by a great increase in the chemical movements and in heat production.

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*CORROSIVE SUBLIMATE IN THE TREATMENT OF CHRONIC BRIGHT'S DISEASES.**

By John C. Peters, M.D., New York.

THIS is comparatively an old remedy, for it was largely used in the New York Hospital as far back as 1842 or 1843. But it is one which experience has shown is exceedingly liable to fall into unmerited neglect, for

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chronic Bright's disease is a disorder which sooner or later must terminate fatally in the majority of instances. Hence all remedies against it are apt to come in disrepute, simply because, however useful and however successful they may frequently be, they will always be summarily thrown aside by some experimenters merely because they do not cure or relieve all cases, under all, even the most unfavorable, circumstances.

I had the good fortune not only to watch the first case, which recovered almost marvellously under this treatment, but many other cases under the care of Drs. John A. Swett, Joseph M. Smith, John H. Griscom, Wotherspoon, Augustus Elliott, W. W. Jones, Israel Moses, Gustavus A. Sabine, and others who were successively attending or house physicians or pathologists of the New York Hospital over forty years ago.

I think, and am in fact very sure, that I first suggested the remedy to my friend and classmate Dr. Wotherspoon, then house physician to the New York Hospital, and he brought its claims before Drs. Swett and Smith, who were the attending or consulting physicians.

The first publication about it was made by Dr. Swett in the *New York Journal of Medicine* for July, 1844. He there gives two cases, first that of Elisha Lynch, who was excessively dropsical, while his urine was not only loaded with albumen, but so scanty that coma was impending. I well recollect the remark of Dr. John A. Swett, "This is a case we will know more about in a few days," meaning that we would see it in the dead-house. Digitalis, apocynum, gamboge, cream of tartar, and elaterium all had failed.

The corrosive sublimate was given, ⅛ of a grain in ʒi doses of tincture of cinchona, every four hours at first, and then, as time pressed, every two hours. It acted decidedly as a diuretic, soon increasing the urine to four pints a day. It removed all the dropsy, except slight œdema of the feet, but much albumen still remained in the urine. Sleep, appetite, and strength returned. It acted as a depurative tonic, and the patient looked well, having greatly improved in flesh and color. Commencing œdema of the lungs had also been removed. Either corrosive sublimate or the cinchona had acted as a blood tonic. The second case was very similar. After the failure of many other remedies, corrosive sublimate was given in ⅛-grain doses three times a day for seven weeks, when the patient was greatly improved in every way. His dropsy was gone, but the albumen remained.

I cannot help pausing here to mark the fact that the remedy was thought worthy of the attention of so great a pathologist, diagnostician, and clinical teacher as was then the late Dr. John A. Swett.

And also to the fact that the corrosive sublimate, with the aid perhaps of the Peruvian bark, acted as a restorative and blood tonic, and depurative.

Whether the mercury merely removed the urea and other blood-poisons, which obtain in uræmia and in some Bright's diseases by means of increased excretion through the kidneys, or whether it absolutely antagonized or neutralized, or in some other way counteracted them, as mercury and iodide of potassium does the syphilitic poison, we know not.

That extremely cautious and painstaking practitioner, the late Dr. Joseph M. Smith, took up the new remedy in 1842, I believe, and possibly before Dr. Swett, but he did not publish anything until 1862. In the mean time, Dr. John H. Griscom, who, like Drs. Swett and Smith, was a consulting physician of the New York Hospital, published two successful cases in 1844 in the *New York Medical Journal*. In one case he commenced with $\frac{1}{16}$ of a grain, but increased the doses to ¼-grain three times a day; they were also given in the tincture of cinchona. Recovery took place, and the albumen disappeared entirely. The other case was notably improved, but was not completely cured. The patient seemed well, but the albuminuria persisted. The same tonic and restorative effects of the remedy before alluded to were noticed.

In 1846 I gathered the experience of the New York Hospital from the house physicians,—Drs. Wotherspoon, Augustus Elliott, W. W. Jones, and Israel Moses,—and from my own notes. All these physicians and Dr. Gustavus A. Sabine, who was the pathologist of the hospital, took great interest in the new treatment, which seemed more successful than any which had been adopted previously. Up to this time about twenty-five cases had been treated, of which five or six had died. Some of the cases which recovered had been very severe, the dropsy was extensive, the urine often almost suppressed, while coma and œdema of the lungs were partially developed. Such cases had usually died promptly under the ordinary treatment, but almost all quickly improved under corrosive sublimate, and often left the hospital apparently quite well. But the albumen and casts disappeared from the urine

in a few cases only, while in some they were diminished very decidedly; in others they even seemed to be increased.

Hence the remedy effected an absolute cure in a few cases only; but it certainly often saved life for the time being. Its most marked obvious effect was an increase in the quantity of urine, which often rose from half a pint to three quarts and more per day, followed by a steady diminution of the dropsy, and gratifying relief from all the most urgent symptoms.

This diuretic effect of corrosive sublimate was then regarded as almost peculiar to Bright's disease, for in a great many patients who took the remedy for other disorders it did not increase the urine in a single case, as far as was noticed or recollected.

About this time strong prejudices were aroused against the use of corrosive sublimate. Dr. Swett, one of its greatest advocates, had died, Dr. Griscom was no longer a hospital physician, and if it had not been for the steady advocacy and even enthusiastic endorsement of it by Dr. Joseph M. Smith, the remedy would have fallen into oblivion.

Dr. Smith did not publish his experience until 1862, in the Bulletin of this Academy for that year. He, as is well known, was also consulting physician to the New York Hospital. He says this energetic preparation has now been used with great success for twenty years in the New York Hospital. He also says it generally induces copious diuresis, and in this way rapidly diminishes the dropsical swellings.

Dr. Bartholow recommends it in chronic interstitial nephritis, when the kidney is small, hard, contracted, rough, granular; in short, sclerosed: viz., when there is hyperplasia of the connective tissue, and that is the principal disease of the kidney. He says he has observed cases in which corrosive sublimate, administered in the small quantities of $\frac{1}{10}$ of a grain, for a lengthened period, effected cures under apparently very unpromising circumstances.

Dr. Loomis is more guarded in his recommendation. He says, "At one time mercurials were extensively employed in chronic parenchymatous nephritis by keeping up its constitutional effects for months. This plan is now partly abandoned, but there are some, however, who claim that the bichloride may be employed. I shall, in considering the cirrhotic kidney, refer to a class of cases in which the administration of this form of mercury is admissible."

He also says, "It has been claimed that the long-continued administration of mercury in small doses has the power to arrest or prevent connective-tissue development; but there is no evidence that it has such power. Besides, in most instances, the cirrhotic kidney is developed in connection with a gouty or rheumatic diathesis, which most positively contraindicates the prolonged use of mercurials. Mercurials can be employed with possible advantage only in those cases in which cirrhotic kidney is developed in connection with a cirrhotic liver. The bichloride is the preparation usually employed in such conditions." We will see anon that Dr. Loomis also recommends it in waxy degeneration.

Dr. Broadbent says, "In the numerous cases of albuminuria in the London Fever Hospital, towards the decline of the affection, when only small quantities of albumen remain, mercury in limited doses usually leads to the entire disappearance of the albumen."

Fothergill says, "Mercury is a remedy which should be resorted to cautiously. While a quite safe measure in the hands of so careful a physician as Dr. Broadbent, it may be a very dangerous agent in the hands of some."

Dickinson, p. 87, says he gives a mercurial purge as often as he sees a case of acute renal dropsy in an early stage, and presumably also in acute exacerbations of chronic cases.

Harley, p. 312, says, "In the amyloid variety of renal degeneration the judicious employment of the bichloride of mercury is often of much use, but salivation should not be allowed to take place. This rarely happens with corrosive sublimate, as it is so soluble that it passes readily out of the system by way of the kidneys. Port wine and nutritious food," he says, "are useful, almost necessary, adjuvants."

Dr. Loomis (see "Diseases of the Lungs, Heart, and Kidneys") says, p. 516, "If amyloid degeneration depend upon syphilis, you will at once make use of antisyphilitic remedies. Iodide of potassium and mercury are the most prominent remedial agents. Both of these remedies have gained some favor in the treatment of Bright's disease, and there are those who employ indiscriminately one or the other, or both of them. The benefit derived in certain cases from their use is undoubtedly due to their power over syphilitic manifestations. In such cases the long-continued use of small doses of mercurials will generally be followed by marked improvement."

As all the preparations of mercury have lately been decreed to act in the same way, viz., as corrosive sublimate, by means of the muriatic acid in the gastric juice, I may be allowed to quote here the use of calomel in heart and kidney dropsy, as given by recent Hungarian and Austrian physicians, who found that 3-grain doses, three or four times a day, were generally followed by abundant diuresis. Sometimes this diuretic action of calomel appeared on the second day, with such intensity that the patients were scarcely able to retain their urine. This polyuria may last from five to fourteen days.

Digitalis, convallaria, adonis, and caffeine are sometimes found unsatisfactory. The use of digitalis often has to be intermitted. Here it is where calomel comes in play. It has increased the urine within a week from thirty ounces a day to one hundred ounces, and in one case from ten ounces to one hundred ounces.

The conclusions are,—

1. Calomel is a prompt diuretic and hydragogue in cardiac dropsy, its action being more speedy than is that of digitalis.

2. It removes not only œdema of the tissues, but also effusions in the great cavities.

3. Diuresis generally comes on suddenly on the third or fourth day, when it is advisable to discontinue the remedy, and only to resume it when the urine again becomes scanty.

4. Œdema consequent upon nephritis, or obstruction to the portal system, and inflammatory exudations, do not appear to be influenced favorably by the mercurial; and even in cardiac dropsy, when the urine is very albuminous, the action of calomel seems to be retarded, no effect being observed in many cases until the fifth or sixth day. It is, perhaps, in these cases that corrosive sublimate, as a more soluble and active preparation, may act more promptly.

Thus we have seen that corrosive sublimate has been found by some very reputable physicians useful in several of the varieties of chronic Bright's disease.

In chronic catarrhal, or tubal, or parenchymatous nephritis milder remedies, like citrate or acetate of potassium, liquor potassæ, etc., may be tried first, aided by buchu, which Fothergill lauds so much. But sooner or later recourse may be had to corrosive sublimate, for which the fluid extract of buchu will prove a good and acceptable vehicle. We have also seen that it is useful in some cases of cirrhosed or even lardaceous kidney, and there seems no special reason why it should not also be restorative in that most common and most important variety of kidney-disease, viz.: chronic diffuse nephritis.

This is multiform and harlequin-like in its appearances. The most common form is that which is often called the "atrophied kidney," which is wrongly decreed always to arise from pure interstitial nephritis.

Another variety is "the large white kidney," which is generally attributed to pure parenchymatous nephritis.

Another and peculiar variety of chronic diffuse nephritis is produced by arterio-capillary fibrosis and by cardiac lesions in the place of the ordinary chronic congestions.

Another common form is that in which the kidney is hardly changed at all in its size, color, or gross appearance. Except for the adherence of its capsule it resembles a normal kidney.

This apparent perplexity, multiform appearances, and confusion are easily explained.

In all these different varieties of chronic diffuse nephritis there are morbid changes in the tubes, the Malpighian bodies, the stroma, and the arteries; but sometimes the disease, or rather chronic inflammation, prevails more in one direction or part than in others. In fact, there is often a great variance in the degree of development of the lesions and the gross appearances of the kidneys.

In some cases the chronic inflammation affects more the epithelium of the tubes and the endothelium of the Malpighian bodies, and then resembles *chronic parenchymatous nephritis*, in which the morbid changes are strictly confined to these parts. Pure and simple chronic parenchymatous nephritis, according to Delafield and Prudden, is really not a very common disease. It is marked by the presence of the large white kidney, and its clinical history, of course, very closely resembles that of chronic diffuse nephritis, which has developed most in the direction of the tubes, parenchyma, and Malpighian bodies.

Again, true, pure, and simple *interstitial nephritis*, marked by great development of the stroma or connective tissue, like that in cirrhosis of the liver, does exist, but not nearly so frequently as it does in connection with chronic diffuse nephritis, which has progressed most in this direction. The atrophied kidney is supposed by some to be the invariable attendant on chronic interstitial nephritis, but the larger number of small, hard kidneys belong to the class of chronic diffuse nephritis.

To recapitulate. There are three great varieties of chronic nephritis:

1. Parenchymatous, generally with the large white kidney.

2. The interstitial, with the small, hard granular kidney.

3. The diffuse or general nephritis, which is the most common and important of all, which may attack the parenchyma and tubes predominantly, and then closely resemble pure parenchymatous nephritis, or it may attack the arteries and connective tissue more particularly, and then mimic interstitial nephritis, with the small, hard, granular kidney.

Corrosive sublimate is possibly a specific against the curable or not too far advanced cases of chronic diffuse nephritis, and there is no reason why it should not be equally useful against chronic parenchymatous and chronic interstitial nephritis.

But there will always be a large number of cases of all three of these varieties of kidney-disease in which no absolute and perfect cure is possible, but only palliation and prolongation of life. Hence too much must not be expected, and a hasty rejection or abandonment of a good remedy should not be indulged in. Richardson says in some cases life may be prolonged almost indefinitely, and the bodily and mental powers retained with so little loss that the patient may be equal to most of the duties and some of the pleasures of life. For the kidney is not equally affected throughout all its parts, and the less injured portions may carry on reasonably well the depurative functions. We must conserve what is left of the kidneys.

Theoretically, corrosive sublimate should be a good remedy against the small, hard kidney, and also against the arterio-capillary fibrosis which so often attends it.

Bence Jones says the salts of mercury act chemically on the organic matters with which they come in contact. The more soluble the preparation the more certain is the action on the different organic substances with which it comes in contact. The solubility of corrosive sublimate in a solution of albumen, like that contained in the serum of the blood, is the cause why this substance is capable of passing quickly into every texture of the body, and when there the same increased chemical and vital action takes place, as we see occurs in the gums and salivary glands as among the earliest signs of the constitutional effect of mercury.

The salivary glands are very different organs from the kidneys, but mercury may act upon the latter somewhat as it does upon the former. Under mercurial action the gums become spongy, the tongue soft and swollen, the salivary glands also softened and enlarged, while they and the mucous membrane of the mouth and throat may secrete mucus and saliva by the pound. All this is very different from, if not the exact opposite of, sclerosis.

It is also possible that it may cause an increased secretion of urine from the kidneys in the same way that it causes pounds of saliva to flow from the parotid, submaxillary, and sublingual glands.

Mercury is one of the few remedies that will reduce and remove the induration of hard chancres, and no disease causes a deeper cachexia than syphilis, which mercury will remove.

Mercury increases all the secretions, including those of the urinary organs. If there happens to be anywhere a recent interstitial deposit of fibrin, or a collection of serum, its absorption is promoted, newly-formed callus is dissolved, and the cicatrices of newly-closed wounds open afresh.

To get the full effect of corrosive sublimate upon the kidneys, salivation must be sedulously guarded against.

The use of mercury in scrofulous and cachectic subjects has always been viewed with some fear. But the orthopædic surgeons of this city use corrosive sublimate fearlessly and very successfully in scrofulous affections of the joints in small or what they call tonic doses, and with such great success that some of them have inferred that more than an usual proportion of these cases are really syphilis in a masked form, so closely resembling scrofula in all its manifestations that a positive diagnosis cannot be made between them.

We may allude here to the large number of cases of chronic arteritis, which are really syphilitic in nature.

Senator, who is almost entirely sceptical about the efficacy of medicines in Bright's disease, approaches this view in a slight degree when he says there is at most (or at least) one drug, viz., iodide of potassium, which must be allowed to possess a certain amount of efficacy in some forms of chronic nephritis. He says he refers to the albuminuria and not to the other symptoms, such as the dropsy and asthmatic attacks, for which we possess really efficacious remedies. Again, in some diseases mercury is known to increase the proportion of red globules in the blood.

Drs. Wm. H. Thomson, Francis Delafield, N. M. Schaeffer, Drs. Boldt, Bunall, and others all spoke favorably of the restorative and tonic effects of small doses of corrosive sublimate in some of the forms of chronic Bright's disease and in some cachectic disorders.

AGAINST THE GERM THEORY OF DISEASE.

By Morris Longstreth, M.D., Philadelphia.

NO subject in medicine during the last ten years has occupied so much thought and given rise to so much research and experiment as the Germ Theory of Disease. Scientists everywhere are busy gaining knowledge concerning the infinitely small organisms suspected of being the cause of maladies; and latterly this subject has become prominent in every household. While the student is searching in his laboratory, with the aid of the microscope, for his microbes, the housewife is ordering the water to be boiled and distributing disinfectants, eating and breathing in fear of the attacks of these little organisms, which she hears that powerful lenses have discovered to be everywhere present. No one has brought this subject so intimately to general knowledge as the French chemist Pasteur, and no one could have been more patient in the minuteness of his experiments.

There have been many theories in past times about the mode of communication of disease which we have not now the opportunity of discussing, and we will simply say that the germ theory of to-day is this: That disease is caused by the action of microbes on our organisms, and that these microbes are infinitely small vegetable growths. Years ago the idea was that disease spread from person to person by a virus, a poison indefinite in character, which had its origin in our bodies, and which could be elaborated either within or without the body, and was spread by immediate contact or through the air. This virus was regarded as a part of our tissues, or was something created as the result of morbid action. Pasteur is trying to teach us that diseased tissues are themselves innocuous and incapable of communicating disease, but that the microbe or bacillus alone is the worker of evil. This is the same doctrine which he promulgated in relation to the fermentations and putrefactions. Liebig held to the old theory in relation to these processes, that it was a transformation or a change of form in the matter which produced fermentation and putrefaction. The introduction of minute organisms has led us to pass from the old theory to a new one for both fermentation and disease. Thus intimately are these two questions connected, and a review of Pasteur's work shows that he passed directly from his experiments in the breweries to his observations and investigations in the hospital wards and the laboratory. In his mind the theory of germ diseases rests on the same basis as the theory of fermentation, and his proofs of the one are of the same nature as those of the other. Consequently it now behooves us to examine his proofs for his exclusive theory of fermentation, in order that we may be in a position to judge of the validity of his experiments in the artificial production of disease.

Pasteur was enabled to formulate his theory of putrefaction and fermentation through his previous studies of the question of spontaneous generation. His attitude towards this latter subject, prior to the commencement of the investigation, was an unfortunate one for arriving at an unbiased conclusion, since, as he himself says, he undertook the work for the purpose of disproving the possibility of the spontaneous generation of the low forms of life which are found constantly present in putrefying and fermenting matters. Is such a mental condition a fair one towards a subject? Is it not always easy to offer an unlimited number of experiments, so arranged as to support and apparently demonstrate a previously formed opinion? His experiments were based on the same principles as those proposed in the early studies of this question, which in the hands of some investigators had led to one conclusion and in the hands of others to an opposite one. The method pursued in these investigations is essentially the following: Take a solution of organic matters, or of some of their components, boil it for a long time, place it in a perfectly clean flask, exclude the air by sealing, and expose the flask containing the solution to a moderate temperature. Pasteur claims that if all proper precautions are taken no evidence of life will be found within the flasks, even after an indefinite period of time; in other words, that putrefaction and fermentation are consequently prevented, and that spontaneous generation is impossible.

Pasteur is much to be admired for the minute care with which he has performed these experiments, and the unsurpassed skill with which he has arrayed the testimony to logically prove his belief. But do his experi-

ments in spontaneous generation conclusively demonstrate the point at issue in respect to fermentations and putrefactions and the vital question involved in the germ production of disease? His theory of fermentation is an exclusive one. He teaches that lower organisms alone are able to produce a fermentation, and, consequently, microbes only are able to set in motion the train of events which we see in the symptoms and morbid changes in our bodies in disease.

The experiments in spontaneous generation are not conducted in the same manner or under the same circumstances as in nature we see putrefaction and disease occurring. Let us consider what are the important differences distinguishing them. First take Pasteur's culture-fluids. These, he insists, must always be dilute, transparent solutions, from which all traces of organized tissue have been removed. These solutions, he says, are sufficient for the growth and nourishment of these low organisms; why cannot these organisms also spring spontaneously from them, if it is possible, under any circumstances, for them to originate *de novo*? It has never been contended by the defenders of spontaneous generation of low organisms that they could arise from nothing, and it would be well for Pasteur and his followers to comprehend fully the difference between nothing and something. Some of the earlier experimenters, not recognizing this difference, employed the gaseous elements of which the lower organisms are composed—oxygen, hydrogen, nitrogen, and a little charcoal—in their culture-flasks, and waited to see if new organisms devoid of parents would spring into existence. Pasteur is a trifle more generous in his ingredients, in that he uses organic solutions possessing a greater expectation of life because once alive. Trials with such weak dilutions do not tell us much in relation to the putrefaction or the productions of diseased conditions of our bodies. Meat or our own tissues should be used. With such matters, instead of dilute infusions, we should be close to the question whether our tissues can produce a poison or virus capable of causing disease, and whether they can putrefy unassisted by lower organisms. With such materials for experiments, we should be better able to decide whether tissues, dying but not chemically dead, are capable of transforming themselves into those lower organisms which swarm in them after their vital energies have ceased. This was the power which the earlier investigators contended did reside in all organized tissues. Needham, the earliest experimenter, called it the vegetative or productive force, and it behooves us to learn whether this force does exist, rather than waste time in trying experiments to see if something can arise from nothing.

In these investigations all recognize the necessity for cleanliness of the experiment flasks. A dirty tube vitiates any experiment, either in chemical researches or in those on spontaneous generation. There is no need for Pasteur to insist on such minutiæ so strenuously, meanwhile disregarding the main point.

Needham's boiling mutton fat possesses a degree of heat quite as efficient for the destruction of the spores of pre-existing organisms in the culture-tubes as the heat of Pasteur's sterilizing ovens; and yet Needham's mutton gravy swarmed with countless organisms. His experiments resemble Pasteur's, save in the fact that the former used a rich organic material and the latter uses a solution of a little sugar candy, tartrate of ammonia, and ashes of yeast, in all not stronger than 10 parts in 100 of water. The heat of the boiling fat far exceeds Pasteur's sterilizing temperature. The difference in the results cannot be supposed to reside solely in the virtue of the sterilizing oven.

It is the fashion, however, nowadays, if an experiment flask shows any sign of life, to say, if there is no discoverable leak in sealing it, that it has not been long enough in the sterilizing oven. The appearance of life in a flask is at once denounced as a "failure." Failures may be due, of course, either to insufficient sterilization, or to a leak. But how is a "success" (the production of life) ever possible if it is always to be condemned immediately as a failure? These men are so convinced of their theory that if a fiat of the Almighty was sent forth calling into existence a new being, it would be unanimously condemned as a "failure," or as "not sufficiently sterilized."

Another point, much insisted on but really of little moment in these experiments, relates to the admission of air to the culture-flasks. In the older investigations, the air was driven out and excluded in hermetically sealing the flasks, and it was claimed the vacuum or the lack of oxygen prevented the growth of the lower organisms, and delayed or prevented fermentation and putrefaction. Various devices were employed to admit pure air, but it has been found that all necessary conditions are fulfilled by placing in the mouth of the

flasks or culture-tubes a large plug of cotton-wool. This cotton admits air, and at the same time keeps out the atmospheric impurities.

A number of fallacies are to be found in the arguments on spontaneous generation, based on the results of admitting pure or contaminated air. It will suffice to point out one only. Claude Bernard devised the following experiment in refutation of Pouchet's observations. The experiment is one familiar to the older workers, and has been imitated by many since, especially by Pasteur and John Tyndall. The object of the experiment is to show that the boiled contents of a flask remain unchanged if the spores of lower organism floating in the air are prevented from entering, or in other words, that if a culture-flask shows signs of life there must have been a leak. Bernard took two flasks containing sterilized fluid, and, after boiling them, hermetically sealed them, and found they showed no signs of life. Then into one flask the circumambient air was admitted by breaking the seal, the flask being subsequently resealed. Into the other flask superheated air only was allowed to enter after passing through a tube filled with red-hot pieces of porcelain, and then this flask was resealed. The result was that in the first flask, with natural air, life made its appearance; the contents of the second flask remained unchanged. The argument from this result is that, as the air is full of the germs or spores of lower organisms, the spores entered one flask freely, while to the other their entrance was prevented, or they were destroyed before entering, by the red-hot porcelain. This argument would be conclusive if Bernard and others had shown, in the first place, that the air contained spores of lower organisms, and nothing else but the spores; secondly, that the spores did actually enter the flask. But we know that the circumambient air contains a great many things; it may contain spores,—they are very difficult to show, however,—and it certainly does contain portions of the tissue of our bodies,—abundance of this material can be found around a man busy at experimental work; it contains quantities of organic matters and inorganic dust. If all these matters enter the flask when the seal is broken, we are not certain to what source to trace the life of the organisms which subsequently appear. If the spores that enter are fecund and find there a suitable soil, undoubtedly some of the life is due to these spores.

But what shall we say of the minute particles of organic and organized tissue which also entered through the broken seal? Are they impotent? These experiments are, and from the start have been, undertaken to find out whether organic and organized tissues were impotent to produce fermentation and putrefaction, or whether out of them could arise new beings devoid of parents. If now, when we have arrived at an *experimentum crucis*, we pass into the flask organized tissue together with the spores, and then say it is the spore which produced the subsequent coming life, and the organized tissue had nothing to do with it, we might as well have left the experiment undone. Such an experiment is inconclusive and quite beneath the acute mental perception of a man like Claude Bernard. It is a wonder the French Academy listened to it.

Mr. John Tyndall modifies this experiment, and it becomes apparently more demonstrative to his mind. This able experimenter constructs an air-tight box with a tightly-fitting door. At both ends he has a small window; in front a large pane of glass. To his audience at the Royal Institution he explains this closet, and, darkening the lecture-room, throws a pencil of rays from an oxyhydrogen light through the two end windows. He calls attention to the minute particles floating in the air, which become visible in the ray of light within the closet, as do the motes floating in the sunbeam in a dark room. After the door of the box has been closed, and these particles have settled to the bottom, he again flashes the ray of light through the windows, and shows that the ray is no longer visible, since the minute particles which reflected the light from their surfaces have subsided to the bottom. Now, to complete his experiment he introduces, through previously prepared openings in the bottom of the box, a number of tubes with open mouths, filled with culture-fluids. The contents of these tubes he exposes thus to what he is pleased to call a pure germless atmosphere, and the contents of the tubes remain without signs of life. But he then opens the door of the box, and it becomes flooded, as he says, with the germs and spores of lower organisms, as he demonstrates again by the ray of light, which fairly dances when the door is opened, like a sunbeam in a dusty room. He believes, and has persuaded many others to believe, that he has materialized the supposed spores, and, with a little imagination to aid us, we see them, almost handle them, as they enter the culture-tubes. And his culture-tubes, previously inert in germless air, soon become filled with abundant life.

This experiment of Tyndall's would be very conclusive, also, were we sure that the motes floating in his ray of light are spores of lower organisms, and nothing but spores. Whereas we are positively sure that there are other things, some of them at least organic, in abundance, and are not at all sure that he has shown us, with all his ingenuity, even a single spore or fecund seed of a lower organism. He has never proved that such spores floating in the air are able to reflect light and become visible in the ray of light transmitted through his closet. In fact, these spores are so infinitely small it is impossible to suppose that they can be rendered visible in this manner. Tyndall, so far as we actually know, might have the air of the closet solid with spores without the ray of light discovering their presence or rendering them visible. This experiment, literally interpreted, would lead us to believe that the lower organisms found in the culture-tubes derive their origin not from pre-existing spores but from the dust floating in air; in other words, that their spontaneous generation was probable, and that they might be beings arising *de novo*, devoid of parents.

Such experiments are mere idle trifling with the real question in hand,—mere tricks of a prestidigitateur. Tyndall seems to stand before his audience like a juggler. In one hand he holds a culture-tube containing a sterilized fluid; with the other hand he gracefully catches his spore, claps it into the tube, and, presto! with the tap of his wand the spore has germinated. The majority of the audience trust him as they do the juggler, that he did what he said he did,—put in only a spore; while others think that the success of the trick depended on the other things put in, and are not at all sure there was any spore present.

I have reserved till now the consideration of the boiling, so much insisted on. All the experiment fluids are what is called sterilized, which means that they have been boiled for an indefinite time. This is done to kill the spores in them. In my course of lectures on the Germ Theory of Disease, before the Lowell Institute in Boston, I objected to the boiling process, simply because by this procedure we cut ourselves off from ascertaining the power or force residing in living tissue. Needham claimed that organized tissues possess certain powers, one of which is to transform themselves into lower orders of life. This he called the vegetative or productive force. This force displays itself during the transition of the tissues from life to complete death or dissolution. Long-continued boiling annihilates this power, and deprives us of the opportunity to estimate this force. The urgency for boiling is very strong in the minds of Pasteur and Tyndall and others, because, as they say, the organized tissues are full of the spores of lower organisms. Of course, no experiment is a fair one if the materials used for culture-fluids contain pre-existing spores. Koch, however, denies that the normal tissues contain lower organisms or their spores, and no one by microscopic examination has demonstrated their presence. Nevertheless, granting, for the sake of argument, that some spores of lower organisms do exist in healthy tissues, it must be conceded by all that the tissues of our body are not made up solely and entirely of such spores. There must exist portions of tissues which are not composed of spores. Now, we know the capacity of the spores: they will reproduce their like. What we are trying to find out is what the tissue will do,—tissue devoid of spores, tissue unboiled and in possession of this vegetative force. This is the problem before us in fermentation, putrefaction, and disease, since in nature things and tissues are not boiled.

A critic of my Lowell lectures, while acknowledging the potency of living organic matter, blundered, as have all the others, in some experiments for testing its inherent power or force. He took healthy blood with all possible precaution, and then *boiled* it. Does he suppose that boiled is the same as unboiled blood? Is the potency of ambient living matter the same after boiling as before? Is a man unchanged in his potencies after being put into a tank of boiling water? If he believes that no change has been effected by boiling, then his experiments are fair; if any change has been effected in the blood, then his experiment is not fair.

Let us perform the experiment in a different manner: Take a piece of flesh from a living animal, say from a healthy man whose limb is to be amputated on account of an injury. The part must be protected from the air by thick layers of cotton-wool as completely as Pasteur protects his culture experiments, and be placed in a rigidly cleaned flask. Do everything which Pasteur does except boil it. What happens? If sufficient heat and moisture are present the flesh decomposes and swarms with minute moving particles, which are called lower organisms, or microbes,—the microbes of putrefaction. But the critic exclaims, This experiment will not do, because the flesh has in it the preexistent spores of the lower organisms. Has

any one shown the spores in such flesh? I do not want to use the flesh if it has such spores in it, but I am not willing to kill the living flesh by boiling for the sake of destroying the spore which is only supposed to be present. The critic says he may be unable to show the spores in the flesh by the microscope, but will undertake to prove their presence in another way, viz., by growing organisms. If an organism appears there must have been spores present, so he says. Here we arrive at the same position again. The critic is convinced of the relationship of spores to organisms, because of his formula, *omne vivum ex ovo*, but he has adduced no proof from his experiment with boiled flesh or blood, because by boiling he has destroyed the vital condition of the flesh in which it can produce organisms, just as freely and abundantly as pre-existent spores can.

If the critic is discontented with healthy tissue, without boiling to kill the spores, for this trial, he ought, if he is fair-minded, to be dissatisfied with the test with flesh after boiling, because he then has to do with dead flesh. The cook in the kitchen could have told him that boiled flesh, with its adherent spores and vital tissue both destroyed, would keep if sealed up. What we want to know is what the vital—that is, unboiled—tissues of our bodies devoid of spores are able to do. If such tissue cannot be obtained the question must remain undecided, because, let it be repeated and remembered, we gain no information from boiled tissues.

Cannot some sporeless tissue be grown, an animal or plant, which has never been defiled with the contact of a microbe of putrefaction? Such tissue could be tested, with fairness, *without boiling* to see if it would putrefy and swarm with organisms. Let me suggest a method of obtaining such an animal. We have all heard of pure cultures of lower organisms. Pasteur's theory of disease-production depends on this method. Pure cultures of lower organisms are obtained by planting, taking the matters containing lower organisms or their spores in culture media suited to their development and growth. From the first culture there is transferred some of the growing organisms to a second, and so on in continuous series until even a hundred generations have been developed. The process is supposed to remove, in the first place, all other organisms, and, secondly, all foreign matters, leaving merely the cultivated organisms. The culture is called pure, because no other organisms are present save the one desired. Could not a "pure culture" of rabbits be obtained in the same manner? Here is an entirely imaginary experiment. Take a series of a hundred air-tight cages, properly arranged with openings for admitting air and passing in food. The air must be filtered from germs, and the food boiled to destroy the spores. The newly-born rabbits can be transferred from one cage to another by forceps or wires which have been properly sterilized. The hundredth lineal descendant of the original rabbit ought to be as "pure" as the hundredth culture of a microbe, if the work is properly done. There need be no more exposure of the successive crops of rabbits to air-contamination than occurs in inoculating the culture-flask with platinum wire when the plugs of cotton-wool are removed; the forceps or wire for transferring the newly-born rabbits from cage to cage can be heated to redness, as is the platinum for inoculation of the culture-fluids, to destroy the spores which stick fast to them. (This heating of platinum wires is part of the legerdemain; I am willing to heat platinum wires to whiteness, if any one will show a spore on such a wire, but I am not willing to deceive myself and others by participating in a trick and calling it a solemn necessity. If there is necessity for heating the wires at all, they should be kept red-hot all the time. After leaving the flame, while passing from the lamp to the culture-flask, and from one culture-flask to the other, the air-spores might alight on the wire, and, horrible thought! contaminate it. To be sure, if the wire is red-hot, the cultured organism will be burned to death, but if the redness is not maintained, contamination is threatened; either way the liability to error spoils the certainty of the experiment.) Here is the way out in regard to the boiling difficulty and the sporeful tissue. Take the hundredth generation of rabbits, kill them by drowning in a sterilized beef-tea, maintain the temperature and the supply of sporeless air, suited to putrefaction. Does any one believe for an instant that those rabbits, thus undefiled by pre-existing spores, having been fed on germless air and sporeless food for a hundred generations, will not putrefy? Does any one think they would remain unchanged forever? If they did, they would form a "monument more lasting than brass" to some future Pasteur. Let no one suggest boiling the rabbits. The necessity for it has ceased; the spores do not exist, and the tissue we do not want killed by boiling it. And now we are in a position to

know what organized tissue devoid of spores can do. Can it or can it not, through its vegetative force, produce the lower organisms, so called, devoid of parents? Can it ferment or decompose without the aid of extraneous microbes?

Who can do this piece of work? It would take a considerable establishment, backed by great wealth, with a corps of assistants, and a judicial-minded, skilled investigator to direct it. The superintendent must not have prejudged the case, he must not start to find proofs that lower organisms cannot arise spontaneously. I know of no one who has the skill, the appliances, and the pecuniary support except Pasteur, but he is lacking in the most essential thing of all,—the open-mindedness. I should be loath to trust the matter in his hands. But until this work has been done we shall not settle the question of spontaneous generation, or know the power or force which exists in our tissues in respect to disease.

Pasteur next turned his attention to the fermentations and putrefactions. On the basis of his studies in spontaneous generation, which had taught him—inconclusively as we have just seen—that no power or force resided in the tissue, he formulated the exclusive theory that no fermentation or putrefaction could take place except from the vital action, the growth of a lower organism. He concluded that, as boiled organic matters were stable until he introduced a fermentative or putrefactive organism, the changes taking place in unboiled matters were due exclusively to these organisms.

I do not question the fact that lower organisms exist in fermenting and putrefying matters; it is their *exclusive* origin from spores which has just been disputed; neither do I question that lower organisms growing in fermentable matters produce certain effects.

The questions which have not been settled are, whether organic and organized matters, other than lower organisms, are in nature in a state prone to decomposition, and are capable, if brought in contact whilst in this natural state, with other organic matters in a stable condition, of communicating a fermenting power or force to such other materials. I question, also, whether Pasteur has shown with entire certainty that the action of the micro-organisms which are present in fermentations is exclusively a vital one.

And to determine this question, it is necessary first to define what is meant by vital action. When an oak-tree is growing in the forest as the micro-organism grows in the brewer's vat, the tree is abstracting from the soil certain elements necessary to its growth. The removal of certain ingredients from the soil introduces a chain of changes which would not take place unless these ingredients were removed. These ingredients can be abstracted by other means than the growth of trees, and similar changes in the soil will ensue. Hence these changes of the soil are not to be regarded as vital changes, and are by no means to be regarded as necessarily connected with the growth of the tree, since such changes can be effected by other means. The vital changes commence only within the tissues of the organisms. So with respect to man: the purchase of meat is not a vital change; those only are vital changes which take place in the repair of the bodily tissues. The depletion or stagnation of the meat-market cannot be looked upon by any one as a vital process. Vital action, then, is such as is directly concerned with vital changes.

Let us look at some very simple and familiar ferments and fermentations. Every one knows something of pepsin and of its action on certain ingredients of our food. The process is so familiar that it is unnecessary to relate it. Can any one show that this ferment's action is due to a lower organism, to a microbe? It clearly is not, and so furnishes a single exception to Pasteur's law, which breaks up his exclusive theory of fermentation. Here is another instance: take a rich solution of sugar or of beef-soup; let the fluids be thoroughly sterilized by continuous boiling until entirely stable, so that indefinite keeping shows no alteration in them. They must be used when fresh, else they insensibly suffer change (to which the name of fermentation has been denied), losing the original characters. Now take your own finger,—this is *not* an imaginary experiment: it has been done,—thoroughly cleansed by means of any or every possible precaution, save that it must not be boiled to sterilization; stop short of that; then place it in the solution of sugar or beef-soup, being careful to admit no spores of lower organisms from the air. After the finger has been kept in the solution for some time, say until the skin begins to bleach, remove it and place the solution in a warm place. What will happen to it after a time? Why, it will rot. The word rot is used advisedly to express collectively the process found to take place in all the fermentations, decompositions, and putrefactions,—use what term we will to express them, as they are all similar, varying merely

in the matters present and the resulting products, and also—as Pasteur would affirm—in the organism which is on hand. And his statement we may credit without believing in the vital necessity of that organism. Does any one affirm that the action of the finger on the sugar solution is a vital action? Certainly it is not so in the sense Pasteur would have us believe that his microbes have a vital action or in alcoholic fermentation. But then it is quite probable that Pasteur does not understand what vital action is. He may have mistaken another sort of action for vital action.

How is the action of the finger on the sugar solution to be explained? If one examines the solution microscopically, one finds in it particles of the outer skin, which, alive when placed in the solution, separate from the skin and float off into the liquid, dying but not dead, only on their way to death and disintegration. These particles being in a state of unstable equilibrium, ready to disintegrate with the heat and moisture, communicate their unstable state to the solution, which had been rendered stable by prolonged boiling. In the solution rendered unstable by the introduction of a pure, clean, but unstable substance, the finger, we have exhibited a certain process. This process differs from Pasteur's fermentations on account of the different agent employed to start the process. The process, however, is of the same nature as his, and is certainly not a vital process, neither is it due to a lower organism.

One word in regard to the method of cleansing the finger. I have said that every precaution was allowable to render the finger clean. The experiment can be readily spoiled and the brilliancy of its teaching marred by the way the cleaning is done. Cleaning is now generally effected by antiseptic agents. Suppose it to be done in the experiment by corrosive sublimate; the corrosive sublimate must be removed from the finger; none of that substance must go into the solution of sugar. Nothing else is wanted to test the merits of a sugar solution and a finger but sugar and a clean finger. This condition of cleanliness towards corrosive sublimate and everything else must be particularly insisted on, because corrosive sublimate prevents every fermentation.

Here is another experiment which it is desirable that Pasteur should try. It contains a vast amount of instruction, looked at from the proper point of view. His theory is that the changes seen in fermentation are due to the vital action of the lower organisms on the fermenting fluid. Could he not grow some pure cultures of the lower organisms connected with a particular kind of fermentation, and then take a handful of them and place them for an instant in the solution? Or, as perhaps the instantaneous immersion would not be needfully long, could he not arrange the flask so that the organism should grow not in the fluid, but in the air on a shelf above it, so placed that the surface disintegrations from the growing fungus—the leaves and bark, as it were, of these little vegetables—would fall into the fluid? By an experiment arranged in this manner Pasteur would be able to tell us whether the fermentation was the result of vital action, or came from the power communicated to the sugar solution by the dying disintegrating particles of the fungus dispersed through it. Until he has told us this, and clearly made apparent this distinction, his theory of fermentation is not worth the paper it is written on. This method of performing the experiment brings it in conformity with the trial of the finger, sugar, or beef-soup solution, and Pasteur will find the result to be the same.

The experiment can be made in another way. Take a piece of tissue, say of liver, kept in a preservative solution in which it will remain indefinitely without change. After a time start a mould to grow in the upper part of the flask in which the specimen is kept. As the fungus dies the disintegrating particles drop down, and coming in contact with the preserved liver, start in motion its hitherto stable constituents, and the disintegrating or rotting process extends to it. Doubtless Pasteur would reject such a method of trial, because, forsooth, the liver had not been "rigidly" enough boiled, or perhaps because it would militate strongly against his theory,—and it is not pleasant to have a theory upset which one has proved so carefully.

The question of fermentation is the reverse one of spontaneous generation. In the latter, organic materials are sterilized by boiling to see if they are then able to originate organisms *de novo;* in fermentation, organic materials are sterilized and then inoculated with an organism to induce a fermentation. Each of these experiments involves an error: in the one is first destroyed the force capable of generating a new organism, in the other the inoculation is made with organisms floating in or mixed with other matters in a state of unstable equilibrium, since it is impossible to separate the organisms

from the matter and introduce merely the organism to the sterilized fluids, and hence we are unable to distinguish which of the two—the organism or the unstable matter—is the real efficient agent in the procuring of a fermentation.

If Pasteur has not shown us conclusively that organized tissues (unboiled) are powerless to cause fermentation, putrefaction, and the like, why may we not believe that emanations from our body in a diseased condition are capable of communicating disease? Diseases have been compared to fermentations, giving the name zymotic to some of them. In disease the tissues of our body become altered, and are separating and being discharged by various channels more rapidly than in health. Why cannot these particles of tissue, coming in contact with another person, be supposed capable of communicating disease, as we have seen the minute particles of finger produce putrefaction in the sugar solution? Why is it necessary, or even rational, to call in the aid of bacteria or microbes? Apparently the only reason for believing in their efficiency and disbelieving in the tissues is that Pasteur has proved that *boiled* tissues are inert. (But these tissues are unboiled.)

How is disease communicated? In contagious diseases, so far as we know, the emanations of the patient, carried by various methods, are transmitted more or less directly from the sick to the well. It is not necessary here to speak of non-contagious or non-infectious diseases, such as gout, because no microbe has been displayed for them, although, for complete consistency with Pasteur's theory of fermentation, bacteria are as necessary for them as for anthrax disease or smallpox. These emanations consist of minute particles of diseased tissue, which float in the air or are in drinking-water, or adhere to articles of clothing, and they can be traced in their departure from the diseased, in their passage and in their arrival to the healthy. These particles of tissue have not been boiled, and why should we not believe in their efficiency for the communication of disease?

Particles of tissue have a very high potency of known character. Look at the pollen of the flower. Look at sperm-fluid coming in contact with an ovum. The pollen and the sperm are particles of tissue not entitled to be called organized, merely organic, separating from the parent organism, desquamating, dying, and on their way to death, but not dead; incapable of sustained separate existence,—and they are not lower organisms, bacteria or microbes,—and behold their power! They possess the greatest power in nature. Can we not believe that a piece of tissue from a smallpox patient, if it has not been boiled, is gifted with at least a minimum of force? Again, if we take a rasping-file and grate off a fine dust of particles of skin over an ulcer that refuses to heal,—it is not necessary to cut out the thickness of the skin or draw blood. Dandruff will hardly suffice; but the mere surface epithelium, which will depart from the skin at the next hand-washing, will serve,—and behold the growth of skin on the ulcer from the planting of such small particles, which we look upon as nearly dead. Can we not suppose that the diseased skin separating from scarlet fever can communicate the disease to the healthy?

This is a show of power, of force of just the sort we are searching for in the communication of disease. Why should we even think for a moment about Pasteur's or anybody else's microbes? Apparently, the answer comes again, because Pasteur has shown that boiled organized tissues are stable, and will not originate a fermentation.

What evidence has Pasteur to offer for his microbe? His proof rests on the artificial production of disease in the lower animals by inoculation experiments. It will be necessary to examine his methods, for he starts much prejudiced in favor of this little vegetable organism, and against organized tissue; in fact, he is ready to hand over the whole of nature to the masterful influence of a microbe. Practically his method of work consists of the same means which he employs in fermentation experiments: merely that he substitutes for the sterilized fermenting fluid an animal into which is introduced the bacteria or microbes. He gets his microbes from the blood or tissues of a diseased animal, and puts a minute quantity of this blood, supposed to contain the microbes, into a sterilized culture-fluid in which, after many trials, the microbe of this disease has been found to grow the best. A portion of this culture, say a drop, is transferred to a second culture-fluid, and this in continuous series for an indefinite number. The final culture is called a pure culture. Just how many transferences or successive crops must be made to arrive at a culture which can be called *pure* neither Pasteur nor any one else has determined. Whether it is the sixth or the sixtieth flask which affords purity no one has shown, or whether the number of cultures differ in different microbes. Each experimenter has stopped his

cultures according to his own fancy, or sometimes when objectors stopped objecting, but as yet the criteria of purity remain uncertain. Some have called the culture pure when no other bacteria can be found in the culture-material save those considered characteristic of the disease.

Let us see what should be the criteria of purity. What is it we want to get rid of? Free from what must the material be to be considered pure? It can hardly be that all these successive cultures should be made merely to free the drop of blood of another organism than the one desired for use. In fact, if the theory which has arisen from all this elaborate work be true, but one organism can be present; if two organisms exist in the blood, there ought to be two diseases, —a disease for each organism. If it is freedom from the tissue that is sought by the culture method, then a lingering doubt exists that the organized tissue may have some force and some power to communicate disease. How is it supposed that the cultures rid themselves of the animal tissues? Pasteur has claimed, or others for him, in his experiments with liquid cultures, that the dilution of the blood by the successive transferences was all that was requisite to rid it of all traces of the original tissue. He has shown that commencing with a single drop of blood, and using each time a given quantity of culture-fluid, and transferring each time a single drop from one culture to another, in twelve successive cultures there is the same dilution of original blood-drop as if it had been placed in a bulk of culture-fluid as large as the earth we inhabit. Such a dilution is so immense, so beyond our conception, that it would seem entirely sufficient for the purpose. Pasteur has contented himself with dilutions less ample as affording the requisite purity. Is the idea intended to be conveyed in this mathematical calculation of the dilution a correct and fair one? I think not, because there are two important factors omitted from the mathematical expression, viz.: the question of the propagation of an influence by the morbid tissue and the time occupied by the process of culture. These two points are very important ones, and to them the attention of criticism has not been previously called that I am aware of. Pasteur, to convince us that his dilution is all-sufficient, speaks as though he had arranged in a row the twelve culture-flasks, into the first of which he places the drop of blood, shakes the flask to mix it thoroughly, and then takes a drop of No. 2 and mixes it with No. 3, and so on with all to No. 12. This would be a fair dilution to the extent he calculates, and the mixture would be effected within twenty minutes. But he omits to speak of the time occupied. In reality No. 12 may not receive its drop of blood for a month or more, because of the pauses made for the culture to be effected in each successive flask. What happens in these pauses? Does anything else take place but the growth of the bacteria? Can anything else be supposed to be happening simultaneously? This culture process is undertaken to show that the original morbid tissue has no influence, and we are questioning whether this method furnishes conclusive proof of its want of power. If the tissue or blood has a morbid power of propagating disease, may it not communicate this power to the culture-fluid and propagate it through an endless number of flasks of fluid, if time be given for its influence to be exerted? If so, the dilution which Pasteur calculates as so enormous is misleading and delusive.

If Pasteur will make the actual dilution he describes, drop the blood into an ocean the size of the earth, and take a syringeful of the mixture for injecting an animal, he will find that not one of the microbes from the original drop will turn up in the syringeful of fluid. The whole question is in respect to the ability of morbid tissue as against microbes to propagate an influence and communicate disease, and the dilutions as performed by Pasteur, with pauses for this influence to be communicated to the culture-fluid, do not prove that this power or influence is eradicated or lessened by the dilution, no matter how great. Dilution of a drop into the ocean will eradicate this power, just as it causes Pasteur's microbe to disappear wholly and completely. Hence we are not certain that the culture method gets rid of the influence of the morbid tissue of the diseased person or animal. Some would say, What about this morbid tissue? We believe (or have believed) that disease is communicated by a *virus*, that so much and no more virus exists in a given bulk of tissue, and that Pasteur's culture method is sufficient to dilute or eradicate this chemical virus. It would be well to give up the use of this word virus. It does not mean anything, and nothing is known about it. It was invented by some poetical gentleman as a cloak for our ignorance. He knew nothing of the way disease was communicated or produced, and he com-

pared the cause to a poison and called it a virus. He never saw it, handled it, or knew anything of its nature: merely supposed it was a chemical compound. A virus can be diluted, while the morbid influence of tissue cannot be eradicated by culture. Let us not mention the word again. Disease is not like a poisoning; typhoid fever in no manner resembles a snake-bite, or morphine- or arsenic-poisoning. The two cannot be compared. Neither is disease like a fermentation. The word *zymotic* is a mere figurative expression, which serves well in board of health schedules, but otherwise is misleading. It does not behoove the germ-theory advocates, who advance the vital action of bacteria in disease, to speak of ptomaines, the chemical poisons which these minute organisms produce, else they will find that the causes of disease must be regarded as a chemical substance,—something to be put into a bottle,—and not the vital effect of a lower organism.

Pasteur has made experiments in a number of maladies and has given us the results of his work. His first achievement was in relation to diseases of the silk-worm. The cause of the decline and death of the caterpillar was clearly pointed out, but that the condition which the author describes is to be compared with such general maladies as smallpox or typhoid fever as seen in the human race cannot be admitted. In one sense the silk-work malady is not a disease: it is merely a mechanical destruction of the silk-worm by a parasitic fungus, which is enabled to grow by attaching itself to the surface of the worm when these silk-spinners are enfeebled by unsuitable hygienic conditions. When the worm is in healthy condition it immediately cleans itself of the parasite as it frees itself from any other foulness. A prisoner in a foul dungeon becomes infested with vermin or gnawed by rats, but the presence of the rats and vermin is not disease. The unshackled prisoner, with soap and water or a club, frees himself from their mechanical irritation.

Chicken-cholera was Pasteur's next work. It was in connection with this disease that his culture-methods were fully developed. For the first time he produced cultures of the microbes having a variable degree of virulence,—some killing the bird within a few hours, others producing death a few days or a week after inoculation. His chief work in this disease was the employment of preventive inoculations. These vaccinations against chicken-cholera have had claimed for them a great success, but the life of a chicken is so short that it is very uncertain whether the effects of the vaccinations are permanent. It has been shown, however, that inoculations with septicæmic matters prevent chicken-cholera as efficiently as Pasteur's vaccine matter, so that it is entirely uncertain whether he ever found a genuine preventive. It is more than likely that Pasteur communicated to the chicken a mild form of septicæmia, which temporarily rendered the bird insusceptible to this disease, as we know that septicæmia renders a person rebellious to the infection of all contagious diseases. Again, it is not improbable, judging from Pasteur's reports, that this disease is not communicable by inoculation, since the symptoms produced in the inoculated bird are almost wholly unlike the conditions seen in chickens which acquired the disease by the natural method.

It is in connection with charbon or anthrax disease in animals and man that Pasteur acquired his greatest fame as an experimenter. In nearly all his observations of this malady he had, however, been anticipated by other observers. His fame arose from the proposal to eradicate the disease by the universal inoculation of every animal liable to the malady. It was the experiments with anthrax which seemed at the time to be producing a furor of belief in the germ agency of disease, and to-day one meets those who, while doubting the proof for other maladies, wonder that a doubt remains of the power of the anthrax bacillus, because, as the remark commonly goes, this lower organism is so easily found and is so abundant in the diseased animal. It seems that belief rises in proportion to the size of the microbe; with a microbe so large,—one we are so nearly able to handle, and one so easily to be seen with even a low-powered microscope,—many yield to the impression that such organisms have a potent effect.

Since these experiments have commanded such wide belief, it is especially necessary to examine the results to see if they prove precisely what they are used to prove.

An examination of the reports of experimenters on inoculations of animals with cultures of anthrax shows varying results, according to the degree of purity from morbid tissue in the culture of fluids. Commencing first by examining cases of disease acquired in the natural method, we find it is communicated from individual to individual, animal to man, by direct contact of the healthy with the diseased, or with the hides or hair taken from diseased animals. The morbid tissue is transferred from one to another, or the currier or wool-sorter has

portions of it lodge on his hands, perhaps in a slight scratch, or he may even inhale a portion. After a varying interval of time, called the period of incubation, a local sore appears at the place of inoculation. This incubation is the time required to pervert the healthy action of the tissues of the hand at the point of the scratch. Then follow symptoms, accompanied by fever, due to the absorption of the unhealthy products of inflammation, and with these symptoms are developed definite alterations of the lungs, spleen, liver and kidneys, as well as other organs. Death or recovery results as in other contagious or inoculable diseases. Pasteur says the period of incubation represents the time required for the growth of the anthrax bacillus and of its dissemination over the body by the circulation. He is not justified in ascribing the communication of the disease to the bacillus, because he has not proved that the morbid tissue that goes with it has not the power of communicating disease. He must remember that in natural contagion the carcass of the diseased animal has not been boiled to sterilization, and that the power of its tissue, greatly enhanced by the diseased state, still inheres.

Now, if we examine the results of artificial inoculations with material from No. 1 culture in the series of a dozen, we shall have very similar results. These will vary, however, with the animal used for inoculation purposes, as it is but natural to expect. They vary because in the artificial case the inoculation may be better, more effectually done; the matter used is fluid, ready to be syringed in easily, instead of a small particle of tissue, frozen and dried on its journey from Siberia, where the disease is prevalent. For these and other reasons easy to suggest, the inoculated animal has the disease communicated more thoroughly; it affects the internal organs more rapidly, and perhaps death may come sooner than in the natural case. If the culture stands a few days, giving the anthrax microbe time to increase in number in the culture-fluid before inoculation, the number of bacilli in the animal inoculated will be found greater than in a person or animal acquiring the disease naturally. But otherwise the symptoms, the changes in the organs, will be exactly the same in the natural and in the artificial disease.

This result does not prove, of course, that the disease-communicating power resides in the tissues, although the tissues were present in both the natural and the artificially produced disease. But let us look at the phenomena in an animal inoculated with the twelfth and later cultures. In such cultures it is evident the tissue has lost its power. Behold the result! The testimony of all observers seems to concur. The animal remains in apparent perfect health about forty-eight hours after the inoculation is made; then, often with great suddenness, crouches down, trembles, or, more commonly, has convulsions, which continue until death, which ensues rapidly. Are those the symptoms of anthrax disease? No, self-evidently not. Let us examine the post-mortem records of the dead animals. The reports read that all the organs of the body present a perfectly natural appearance, save that in many places, especially in the lungs, minute blood-spots, called ecchymoses, are seen. An examination with the microscope shows in the centre of these little spots a minute blood-vessel blocked with the anthrax bacilli. This blocking has caused the rupture of the blood-vessel, and the blood and the bacilli are poured out into the tissue. Hundreds—nay, thousands—of the blockages with hemorrhages are to be found, but especially numerous in the lungs. Are these the changes which are found characteristically in animals dead of anthrax disease acquired in the natural way? Again, no. What have the two results in common? Apparently only death. Can two diseases be considered for a single moment the same or in any way related which have neither the symptoms nor the alterations in the organs alike? Again, most emphatically, no. I said just now that the two conditions—inoculation with a No. 1 culture and with a No. 12 culture—have death only in common. They have also the bacilli in common, but the number of these organisms in the two conditions differs, the microbes being vastly more numerous and more concentrated in the vessels in the latter than in the former. They differ in another respect, also, viz., their cause or antecedent circumstances, and this fact is most important to remember. In No. 1 there was used for inoculation tissue and bacilli, and in No. 12 bacilli without the tissue. We have seen the difference in the result: one produces the disease, the latter produces death without the disease.

How can the result be accounted for from the bacillus inoculation with the No. 12 culture? Very readily, indeed. Into the animal we syringe a small quantity of fluid containing nothing save a richly-growing organism. During twenty-four or forty-eight hours the

microbes found in anthrax disease increase immensely in number; the few that were introduced have become millions on millions; and note especially that the animal inoculated shows no deviation in health, either generally, or locally at the point of inoculation. It eats and drinks and frolics, as it ought *not* to do if millions of anthrax bacilli can produce injury by their vital action. Suddenly the scene changes: out of perfect health panting and convulsion come. Can we find from the post-mortem examination anything to account for this sudden convulsion and shortness of breath? Pasteur says the sudden death is due to the anthrax microbe consuming by its vital action the oxygen of the blood, and the deprivation of oxygen causes the convulsion, and, as the process of absorption becomes complete, death. But what has the post-mortem examination shown us? It has shown blocks, called emboli, composed of bacilli, in half the minute vessels of the lungs. These blocks have stopped the circulation of the blood through the lungs, and consequently prevented the aeration of the blood. We know of death with similar symptoms produced by another condition of the blood-vessels of the lungs in which bacilli are not present. A woman after childbirth has one of the clots naturally formed in the uterine veins detached and carried by the blood-current, first to the heart and then to the lungs. When the clot blocks the vessels of the lungs, the woman, from a condition of perfect health, passes into convulsions, and with panting respiration dies very quickly. The symptoms and the post-mortem examination show precisely the same conditions in the animal inoculated with a pure culture of anthrax bacillus and the healthy woman in childbed, save in the former the occlusion of the blood-vessels is due to collections of anthrax bacilli which have lodged, and in the latter the blocks are composed of firm clots of blood. In these two instances death has no more been due to anthrax disease—or, in fact, to any other disease, as the word is ordinarily used—than in the case of a man on the scaffold with a rope around his neck. They are all instances of strangulation. The man's death we know all about, for we are familiar with ropes. The woman's death is more recondite, but entirely simple to those familiar with such conditions. In the inoculated animal we are not prepared to believe that anthrax disease has been communicated because we are shown a dead animal and the bacilli.

One word is demanded to explain that the capabilities of the anthrax bacilli of killing in this manner are possessed by this microbe alone. The anthrax bacillus has a length greater than any other lower organism found in connection with internal diseases. Its length exceeds the diameter of a red blood-corpuscle; hence all goes well when they are carried endwise in the blood-current, like logs of timber carried down-stream; but in the fine capillaries, which can carry only a single red blood-corpuscle, let an anthrax bacillus become turned crosswise, and immediately a jam, as of logs in a river, takes place, the bacilli become piled up, and the blood-vessel ruptures and causes a minute hemorrhage. On account of its superior length the anthrax microbe alone can furnish such an incident and produce, when in sufficient numbers, this sudden death. Hence the brilliancy of Pasteur's results,—sure death to the inoculated animal,—together with the readiness of displaying the microbe, has gained him many believers.

In respect to his preventive inoculation of anthrax disease, we cannot stop to discuss his juggling to prepare the vaccine matter, which used to be offered for sale by the kilogramme at Boutroux's shop, Rue Vanquelan, 18, Paris; it must suffice to say that it is an easy deception of observation readily explained. Whatever else is done in preparing the vaccine material by cultures, it is evident from Pasteur's own claims that the virtues have so far departed that the disease itself is not communicated; it is also true, if his own theory is to be trusted, that a rotting fluid is prepared,—using the word rot in the broad sense, decomposed in some manner by the growth of a microbe,—and this rotting fluid is used for the preventive inoculation. The expected conditions follow; a mild form of septicæmia in the cattle is caused, rendering the animals "refractory" to the influence of the contagion of anthrax. Pasteur will find his animals equally protected against the smallpox or scarlet fever or any other contagious malady to which they may be liable. Man is not likely to contract smallpox or other contagious diseases while suffering from septicæmia. Septicæmia is a more sure protection from contagion than the contagious diseases are against each other. Measles and scarlet fever, and other eruptive diseases, may follow each other in rapid succession, or may coexist even, but septicæmia while it exists, and for long after, bars out other maladies of a contagious sort. But Pasteur does not seem to recognize these facts any more

clearly than he comprehends that his preventive inoculation is a septicæmic condition. Why should we expect him to? He is not a physician, much less a pathologist. Why is it not more than likely that he should be self-deceived by his experiments? Further, the inoculations are protective for only a few months. When the septicæmic condition of the animals passes over, they are liable to the disease as markedly as before, and so the whole matter has been given up.

The next group of diseases, for which proof has been offered of their causation by microbes, is the surgical infective diseases. This work finds a prominent representative—and at every step other observers have rendered contributions of an important character—in Dr. Koch, of Berlin, who has since made us acquainted with his studies in tubercular disease, and more recently cholera. With these diseases, which include septicæmia, pyæmia, and erysipelas, it is difficult to discover on what grounds the proof of their bacterial source is founded. It is not contended that the maladies are specific contagious diseases in the sense that smallpox and scarlet fever are. One case does not presuppose an antecedent attack of disease in another person, although such may be. The cases originate *de novo*, and do not have to be transmitted through a continuous series. Their nature is infective, and they arise principally from or through neglect or uncleanly conditions of wounds, although they may acknowledge another source.

The variable or *de novo* origin of these diseases has been taken advantage of by observers in studying their cause and nature. The experiments are begun by injecting into the tissues of animals putrid meat infusions. From animals which die of the putrefactive infective disease portions of the fluids or tissue are taken and inoculated into another series of animals, and so on, with the result, which is not at all surprising, that the virulence of the infectious matter is increased, and death follows the inoculation with greater and greater rapidity.

It is at once very striking that this method of study differs *in toto* from that in other diseases. In all the others the aim has been to arrive at a pure culture of the germs or microbes. Why is this difference? No explanation has been offered, and yet it would seem we are equally in these diseases entitled to see, to study the action of the pure culture of organisms, apart from the effects of putrid tissue, as in the other maladies. Nay, is it not far more important, since infective diseases are not specific, but can be generated from perfectly indifferent substances, such as putrid meat? Hitherto we have believed that wound infection was due to the absorption of the putrid discharges derived from the rapid breaking down of tissues; and now, as the result of these experiments, using for infection putrid meat,—and the organisms in it,—we are invited to believe that the efficient cause—the *causa causans*—is the organism alone, and that putrid tissue has nothing to do with the result.

I do not see how it is possible to attain to a pure, that is, a clean culture of these organisms. If the function of the microbe is putrefactive, the hundredth transference in the ninety-ninth culture-flask results in putrefaction of the culture-fluid, just as surely as this process is going on in the original putrid meat infusion. Injection of putrid culture-fluid teaches us no more than putrid meat infusion. In both cases two things are injected, a putrid tissue and certain organisms; and the question still faces us, Which of the two causes the disease?

The argument which the Germ Theorists have been pleased to put forward runs as follows,—the argument holds for this group of diseases as well as for the others: "I discover the microbe, I cultivate it, I inoculate it, and I reproduce the disease" (*Trelat. Soc. de Chirugie de Par.*, 21 Oct., 1885). One portion of this argument without all the others does not lead us to any conclusion, and the mere finding of an organism in the dead or living tissues of disease furnishes not an atom of proof; nay, not even a probability or a possibility that it has anything whatsoever to do with the production of disease. A great deal has been written about organisms being "constantly present," or being "characteristic" of a disease, and, in fact, quite too much has been said. So very much depends on how one is looking for organisms. Has the search always been made in the same manner? Have all sorts of methods been used to discover all varieties of organisms? Or has the organism been called "characteristic" to meet the observer's wishes? Reviewing the history of organism in disease, we shall find that three other organisms have been pointed out in tubercular disease besides the present reigning one, Koch's tubercle bacillus. Pasteur has presented to the French Academy three different microbes as the genuine cause of hydrophobia. These several organisms have been in turn

abandoned in favor of the newly-discovered one, and we seem to be in daily dread of a new announcement, a new introduction which may upturn our settled convictions. The abandoned organisms do not disappear; they can be found by looking for them. The discovery of an organism does not present anything "characteristic" for the disease, in fact it gives us nothing but the opinions of some one or more observers, and we may be only on the brink of the invention of a new microscope to see a new bacillus hitherto invisible but equally constant with the present one.

The cultivation of the organisms—the second factor of the argument—we have seen; we know the process, and know what it amounts to; we have seen that it is carried on to produce purity; we have seen the uncertain standard of purity. The method may furnish us with microbes of one variety, but it never gives us purity; it never gives us singleness. We always end with using two things in inoculations, the microbe and the culture-fluid, and we shall never know to the end which one of the two produces the result. If it were possible to impale a microbe on a needle, to wash it under the faucet and rub it dry with a clean towel before inoculation, we should then know what the microbe was able to do when growing in our bodies. By this means we should see if its growth were able to cause the symptoms and the post-mortem appearances characteristic of the disease. We should have placed before us the genuine effects of the growth of the low organisms, and we should have purity, genuine purity, in our culture. As it is now, we know no more than if we took a retort with a piece of copper in it and poured on it a mixture of an acid and alkali and said, Behold how the acid affects the copper! To-day we are doing a parallel piece of work with the inoculations. We inoculate with microbes and the culture-fluid in mixture, and put the whole result down to the credit of the microbe.

Again, in judging of inoculations, a result which satisfies one observer that he has reproduced the disease does not convince others. Many conclude, when they succeed in killing the inoculated animal, that the death shows a sufficiently evident result; this is especially conspicuous in Pasteur's works; the death seems all-sufficient. Many experimenters have employed in their inoculation animals which are not susceptible of the disease under any circumstances. How can any faith be placed in their results?

The question of tubercular diseases is a most interesting and important one. Koch, of Berlin, has been the chief to formulate the germ-teachings—in fact, the discoverer of the bacillus—of this group of diseases, but a whole host of experimenters have repeated his observations. It seems almost impossible to point out, in less space than a volume, the fallacies or lack of proof of these observations, and quite hopeless to make all the objections to them clear in a few paragraphs.

In 1865, when Villemin showed by experiments that it was possible to cause tubercles to appear in animals inoculated with certain degenerating organic matters, the general opinion held at that time was that tubercular diseases were occasioned by these same materials originating from a variety of conditions within the body of a person or animal attacked by tubercle. The passing strange part of Villemin's work was that the inoculation of foreign matter served the same purpose in causing the disease as the matters occurring naturally within the body. Villemin's observations were repeated by other experimenters in a great variety of ways, and were in the main confirmed. But another important fact was shown by the experiments of others which Villemin did not discover. Villemin claimed that only tubercular matters could communicate tubercular disease to the inoculated, and that therefore tubercle was a specific disease, since like produced like. But his imitators showed that unlike, dissimilar, and even indifferent organic matters were capable of producing tubercular disease in the inoculated animal. A lump of cheese was found by many experimenters to be just as effective in producing tubercular disease as a piece of tubercular lung when introduced beneath the skin of a rabbit. In other words, any degenerating inflammatory product was liable to produce tuberculosis, and therefore the disease is not truly a specific malady, and although like did produce like, unlike matters could also produce it. Here the matter rested. The question at this time did not stand in relation with the germ theory of disease, and very rash would have been considered the theory to announce that consumption was a contagious malady because it could be inoculated.

Before the introduction of Koch's bacillus of tuberculosis there had been, as has already been mentioned, three other organisms observed, which were handed around Europe for inspection and vaunted as the genuine cause of tubercle, but, because of the minute care

which Koch had observed in his experiments, his bacillus was at once accepted as the only genuine.

And now, if this bacillus was to be accepted as the cause, it became necessary for the eminent pathologists who had found cheese efficient for the production of tubercle to explain the discrepancy. A few, Cohnheim among the number, suggested that the cow furnishing the milk for the cheese was probably tuberculous. No one ever showed the presence of bacilli in cheese; in fact, cheese manufactured in the ordinary way could not have bacilli in it: they would be destroyed; nevertheless the argument was made with seriousness. Others concluded that the results from cheese were due to bacilli, accidentally floating in the air or adhering to the knives and forceps, inoculated along with the cheese. And to remove this accidental contamination of the cheese they repeated the experiments with all due precaution. What were these precautions and how did they affect the result? They were the usual antiseptic surgical means, consisting of the use of carbolic acid spray and solution in which the hands and instruments and the wound are washed (I don't know whether the cheese was soaked in it or not, but it ought to have been, according to their theory, if it was not). Now, having inoculated the rabbit with a great deal of carbolic acid and cheese, they came to the conclusion that cheese was inert: that it never did or could produce tubercle. What a sorry piece of evidence to offer! Suppose Koch, when inoculating rabbits with pure culture of tubercle bacilli, was to do it in an atmosphere drenched with carbolic spray, his hand and instruments dripping with carbolic solution and the wound of inoculation flooded with it, would he expect his bacilli to live through such a flood and cause tuberculosis in the animal? He never allows carbolic acid to come near the bacilli, and if he did he would have negative results. No, it is the same old fallacious method of dealing with the question. Start out with the notion that lower organisms are the only genuine cause, and living tissue (or cheese unboiled) cannot possibly have any effect, and an endless number of experiments can be arrayed to favor the view. Do everything to favor the microbe, and do everything to render the tissue inert,—boil it, flood it with carbolic acid or corrosive sublimate, give tissue no chance at all,—and you will arrive at the same conclusion. Use cheese or other degenerating organic matter without bacilli and no carbolic solution, and you will have a different result. Do not kill the bacillus (and the cheese along with it) before you catch it, or else try a boiled bacillus as well as a boiled cheese, and see where you will come out, since either both should be boiled or neither.

It is therefore yet possible that some may find a way to show the efficiency of cheese or other organic matter free from bacilli,—and there is plenty of such matter to be had,—and then the bacillus of Koch will lose its significance and importance. But let us consider the fallacy lurking in Koch's experiments. This gentleman has produced the pure culture of the bacillus in the usual manner by successive generations. The culture medium he has found necessary for the growth of the organism is a preparation of gelatinized blood-serum,—a solid medium instead of the fluid. These cultures are called pure, as in other cases, because no other organism is growing in them. But, as in other cases, the mass actually employed for inoculation consists in a considerable proportion of the gelatin, with the bacillus growing in it,—two things. The blood-serum, an organic matter previously sterilized into inertness by boiling, is passing, through the agency of the growing organism, into a state of decomposition, degeneration, rotting,—call it what you will,—and is no longer sterile, but resembles the state of cheese or other organic matters which are capable of producing tubercle. That Koch's gelatin contains bacilli is a matter of indifference. That the bacilli are mixed up with rotting gelatin is a matter of great difference, since we are thereby prevented from knowing what bacilli alone can do, unaided by a material in itself capable of producing tubercle. The reply is made to this criticism by those who do the experiments that "control" inoculations show the blood serum or gelatin to be wholly innocuous. They take blood-serum from the tube in which the bacilli grew, merely half an inch away from the growing organism. No one contends that blood-serum is noxious when sterilized, or that the serum within the tube, except as it comes within the influence of the growing organism, becomes unsterilized no one affirms. But that the serum or gelatin on which and in which the tubercle bacillus grows is no longer sterilized every one must admit, and it is this actively changing serum or gelatin which is used for inoculation. To say that blood-serum or gelatin sterilized, even if taken from the same tube with the growing organism, is harmless does not tell us anything useful,—for with the microscope,

or even with the naked eye, it is plainly evident the serum is not in the same state as that in contact with the growing organism, and which makes up the bulk of the material used for inoculation. It is probable, on the contrary, that serum or gelatin undergoing such rapid degenerating changes, induced by the growing organism on its surface, is more powerful to cause tubercular disease than other decomposing organic matters occurring either within or without our bodies; as it is thus rendered specially potent, less bulk of it serves to produce tubercular disease in the inoculated animal, irrespective of the presence or absence of the bacilli. The tubercle bacillus of Koch is but an epiphenomenon of the disease. What is wanted for use in inoculations is the bacillus alone, freed from everything else. If, again, Koch will impale a bacillus on a needle, wash it clean, and introduce it, then we shall know what it is able to do. We object most strongly to the mixture of rotten gelatin and bacilli, for the rotten organic matter we have hitherto believed to be the cause of tuberculosis.

The claims which have been put forward for the germ origin of cholera are very uncertain. Two great government commissioners have investigated and reported, and still the question is more than uncertain and unsatisfactory, and it must be very unsatisfactory even to the commissioners themselves. In fact, so little has been shown conclusively that there is scarcely anything on which to base a criticism. A majority, perhaps, of those who would admit the claim of a germ origin, were it once established for other diseases, would deny its possibility for cholera, such is the dissimilar character of the malady.

Finally come Pasteur's studies in hydrophobia. This illustrious man has made three attempts at the malady. The first essay was in 1880–81. It was a microbe taken from the saliva of a child dying of rabies after a bite from a mad dog. This was a very evident mistake, which, however, the French Academy applauded until it became quite generally known that inoculations with this microbe merely produced septicæmia. A few years later, in another communication to the Academy at Paris, another set of conclusions was presented, to be again received with favor, only to be abandoned, without disproof of any sort, for a newer theory of the distinguished savant. This last theory—the procedure under it has been officially published in the *Herald*—differs *in toto cælo* from the previous ones, and is in fact a virtual actual abandonment of the germ theory of disease. All of Pasteur's former work was to establish the fact that a microbe was the sole agent in producing disease, and, in fact, for effecting every other change in the universe.

His latest work in hydrophobia discards the micro-organism and substitutes the diseased tissue of the rabid animal. Pure cultures are thrown to the winds, and preserved spinal cords and brains, without a word of microbes, which cannot be made to appear effectively, are brought forward to do the work.

What is the method of procedure? The spinal marrow of a rabid dog is mingled in definite proportions with pure veal-broth, and this is injected into the brains of a rabbit. This experiment, Pasteur assures us, communicates hydrophobia to the rabbit. By successive transmissions from one rabbit to another the virus becomes modified, and thus fitted for the inoculation of persons in order to prevent the development of hydrophobia in those who have been bitten. In inoculating with the attenuated virus those threatened with the disease, the primary injection with the mixture of spinal cord and veal-broth is made with the weakest mixture and continued for ten or eleven successive days with a graded mixture becoming progressively more virulent. The final injection is said to be performed with a rabic virus which will produce hydrophobia in an animal or in a person unprotected by the previous injections in six days, a period shorter by one-half than the virus taken directly from a rabid animal's spinal cord would require to produce the malady, or in a very much shorter period than such an animal's bite would take to turn one mad.

In this description—and the details of the method are all kept secret—not a word occurs about the microbe; and in fact it is certain, since the spinal cord itself is employed, that the effects cannot be due to microbes alone, even supposing them to be present in the tissue. With these experiments before us one is almost tempted to believe in this item of Pasteur's work. He makes use of diseased tissue, which one could rationally suppose was able to communicate disease. That the diseased conditions reside in the nervous tissue alone we have not a single item of proof, and there is not a particle of reason to believe that this is true. Would not a piece of the tongue or a toe of the dog serve equally well? One can hardly be brought to believe that a general disease like hydrophobia is localized

in one portion of the body. Let us be rational for once.

But apart from the method of preventive inoculations of hydrophobia which Pasteur has invented, and of which we as yet know scarcely anything, the point to be insisted on in connection with our present discussion is the complete abandonment by him of the germ theory of disease. Without a microbe to look to where are we, and what becomes of that theory? If we are no longer even trying to, deal with pure cultures of organisms, but purposely include diseased tissue in our inoculation material, the theory vanishes, and we come back to the old notions, putting our faith again, as is rational and in accordance with endless testimony, in the potency of diseased tissue for the communication of disease, and in nothing else.

FLUID EXTRACT OF CORN SILK (STIGMATA MAIDIS) IN THE PRIMARY OR ACUTE STAGE OF GONORRHŒA.

By E. Stuver, M.Sc., M.D., Rawlins, Wyo. Terr.

EVER since engaging in the practice of medicine, over six years ago, I have been a constant reader of from four to six of the best medical journals published in the United States, and always having a large number of cases of gonorrhœa to treat, have eagerly watched for every advance made in the treatment of this—in many cases—obstinate disease; but while almost innumerable remedies have been suggested, and their merits discussed pro and con, I have never seen any reference to corn silk, which, in my hands, has given such satisfactory results that I have decided to call the attention of physicians to the remedy, and ask them, after having carefully tested its merits, to report the results obtained from its use. Right here I want to insist on the necessity of using a perfectly fresh and reliable preparation of the drug, as there is the greatest difference between the physical qualities and therapeutic action of the various products put on the market, even by our most reliable manufacturing pharmacists.

I began using corn silk in irritating and painful affections of the kidneys and bladder, attended by dysuria, about five years ago, and in the *Medical News* of October 6, 1883, published an article on its "Anæsthetic and Diuretic Effects." In this article special attention was called to the beneficent influence it exerts over the irritable condition, attended by frequent micturition, so often encountered in pregnancy.

Encouraged by the results obtained in the above-mentioned class of cases, I decided to test its merits in allaying the irritation and pain of the acute stage of gonorrhœa. A case soon presented itself, and the following condition noted:

C. H., laborer; contracted gonorrhœa a short time ago; discharge made its appearance yesterday; inflammatory action high, and pain on micturition severe. After giving the ordinary directions as to the regulation of diet and the alvine evacuations, abstention from spirits, etc., I gave the following, viz.:

℞ Potassii acetatis, ʒiii;
Fl. ext. stigmatæ maidis,
Aquæ destillatæ, aa ad q.s. fʒiv. M.
Sig.—ʒii in water every two hours.

The following day he reported himself as greatly relieved, and under this treatment not only did the irritation and pain subside in three days, but the discharge almost entirely disappeared, and he felt so well that, against my positive instructions, he resumed his work (digging a ditch), and had a recurrence of the symptoms, which did not yield as readily to treatment as in the first instance, but were controlled, so that the entire attack did not last more than two weeks. A large number of cases might be cited, but, as they have histories very similar to the one above related, I refrain from giving them. I do not want to be understood as claiming specific powers for corn silk in the acute stage of gonorrhœa, but merely desire to call attention to it as a remedy of positive value in properly selected cases.

KAWA IN GONORRHŒA.

Dr. Sanné recommends in the *Bulletin Général de Thérapeutique*, March 15, 1886, the kawa-plant for the treatment of acute affections of the urinary passages, especially of gonorrhœa. He gives four to ten pills of the extract daily, each pill containing $\frac{1}{6}$ grain of the extract.

The employment of kawa for these affections does not appear, however, to be of a recent date, for Gobley and O'Rocke advocated the use of this drug as early as 1860 in the same French journal.

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Leading Articles.

ANTIPERIODIC EFFECTS OF QUININE.

SCIENTIFIC therapeutics is something like a family flour-barrel: everybody goes to it and everybody gives it a kick. Very frequently, however, the cause of the grumbling lies not with the investigator of physiological therapeutics so much as with the pathologist. In many places practical therapeutics outrun pathology, and the results which we obtain at the bedside by the use of remedies cannot be explained, because the pathologist is unable to explain the rationale of the disease.

The antiperiodic property of quinine has long been an absolute mystery, which has baffled every attempt of the scientific therapeutist to solve. It looks now as though the problem would be an easy one when the pathologist has done his work. The recent studies made at the Philadelphia Hospital by Professor Osler strongly strengthen the probability that the cause of intermittent fever is an infusorium in the blood, as first noticed by Thomasi Crudelli. Professor Osler has examined some forty odd cases, and, with scarcely an exception, has found the creature in one of its three forms, either as a flagellate living body, moving about the red blood-corpuscles and scattering them hither and thither, or a quite crescentic-shaped body, or an amœboid form living in the red blood-corpuscles, and apparently destroying them.

In one or two cases in which the diagnosis founded upon the symptoms was very doubtful, Dr. Osler was able to decide the nature of the case by finding these creatures in the blood of the patient. In all of such cases quinine produced rapid amelioration. Quinine acts as a powerful toxic agent upon many of the lower forms of life, and the theory must at once suggest itself to every mind that it cures intermittent by destroying the germs to which the fever is due. We do not know of any direct experiments upon these infusoria with quinine, but certainly such studies are urgently demanded. Professor Osler has found that the cure of the intermittent by quinine is followed by the disappearance from the blood of the infusoria; such disappearance not, however, being rapid but gradual. No one has yet succeeded in growing the infusoria outside of the human body.

Although the suggestions which have been put forth in this note still belong in the category of speculation, there is enough foundation of ascertained fact to stimulate very greatly the study of the matter by those who are situated in highly malarial districts.

BROMIDE OF ETHYL.

IN a recent number of *Science* there is recorded in detail a death which recently occurred in Brooklyn or New York from the use of bromide of ethyl by a dentist. The rapidity of action, and the general comfortableness of the bromide as an anæsthetic, leads every now and then to its being recommended by some one, and to its still being used to some extent.

When it was first actively proposed as an anæsthetic it was carefully investigated by Dr. H. C. Wood, who, as the result of his experimentations on the lower animals, arrived at the conclusions that it acts like chloroform on the heart, and that it would be a more dangerous anæsthetic than chloroform. Within a very few days after the publication of Dr. Wood's paper a case was reported in which death from cardiac failure was averted with the greatest difficulty, and only a few weeks later a fatal anæsthesia was put on record. Since that time there have

been several deaths from the use of bromide of ethyl. Those, too, in spite of the general abandonment of it.

There is probably no anæsthetic which has ever been used to any extent that has as high a mortality rate as has the bromide of ethyl. The advantages which it possesses over chloroform are so slight as to be of no importance. We do not think the habitual use of chloroform as an anæsthetic is justifiable, but certainly it is much better to employ it than to venture the dangers of the bromide.

PRACTITIONERS AND SCIENTISTS.

IN a brilliant review of the recent book of Mr. Whipple's, which contains the lives of Choate and Agassiz, we were much struck by the remark that the work of the lawyer is necessarily fugitive and without permanent record of fame, because he simply labors to convert his intellectual abilities into money. His work is temporary and is rewarded immediately. He builds no more for the future than does the man of business or the man of pleasure.

It is affirmed of Agassiz that he once said that he had no time to make money. This was because he was building for the good of mankind and for future generations. The best work of the world is never paid for. Newton, when he labored on his immortal astronomical papers; Keppler, when he discovered the laws which control the universe, never dreamt of pecuniary reward. The man of medicine stands midway between the lawyer on the one hand and the devotee of abstract science on the other. He partly works for money and he partly labors for the universal good, and just as he works for the one or for the other so is his reward. The fashionable practitioner, the great specialist, who annually receives his many thousands of dollars, is the man of the present. Often, however, his present success is founded upon his past labors for science, and thus it is that very few original great medical works, and very few great discoveries, very few profound researches spring from men who are past middle age. The scientific work of Charcot was done in the days of his earlier manhood. The labor of his later years goes to maintain a gorgeous palace and a retinue of servants.

American scientific medicine especially fails in its highest development on account of the ease with which men great in medical science slip over into the ranks of the money-makers. The naturalist who for ten or fifteen years has done good work finds as he approaches forty, the age at which avarice begins to assert itself, and the demands of an increasing family grow clamorous, no new avenues open to receive him by means of which he can rapidly make money. But the American physiologist, or man great in medical science, at forty begins to see avenues leading down into new vistas of financial prosperity opening themselves in pleasant sunshine about him, and so forsaking the nobler labor of building for the general good and for the future, fattens himself in the fields of popular patronage. Thus it is that whilst American naturalists stand foremost in number as well as in greatness, American medical scientists, whose names deserve to be put alongside of those of the great men of Europe, are so pitiably few.

CERTIFICATES TO PROPRIETARY MEDICINES.

WE print in another column a letter from one of our subscribers concerning the giving of certificates by physicians recommending proprietary medicines. We sympathize with the views expressed in that letter thoroughly. There is no simple thing connected with the medical profession which conveys an impression of besotted weakness so strong as that produced by the certificates which are to be seen signed by men of prominence.

We are very sure that the physicians who sign these certificates are never paid for the use of their names; if they did receive an honorarium commensurate with the service which they render the proprietor of the medicine, we would have a less opinion of their morals, but a much higher one of their common sense, self-control, and business tact. The smooth-tongued, wily commercial traveller usually holds the doctor in the hollow of his hand, and moulds him as plastic clay. After his return to his rooms in the night, he enjoys most heartily recounting his triumphs of the day with his fellows.

It is very rare for a doctor to thoughtfully and carefully study the effects of one of these proprietary medicines. He gives them; the patient gets better, and he signs the certificate, receiving as an honorarium a bottle of the wretched stuff, which he in turn bestows on some poor patient. Probably the other medicines and hygienic treatment which

accompanied his gift of the sample were responsible for the good achieved, but the doctor fails to recognize this. One thing is positive, that these proprietary medicines contain nothing which may not be equally well represented in office prescriptions, and are charged for at a rate which is exorbitant. The advertising agent and commercial traveller, the medical and the lay journal management, all have to be paid out of the profits of the proprietary medicine, and the seller of this medicine deems himself a shamefully abused creature if he does not make two or three hundred per cent. clear of all his expenses.

In no section of medicine has this abuse of certificates been greater than in regard to disinfectants. There is probably no disinfectant proprietary solution prepared or sold and placed upon the market which in any way represents the price at which it is sold. Most of these proprietary disinfectants are almost inert, and the most worthless of them are those to whose certificates are appended the names of the greatest of our physicians. A surgeon uses listerine, or some other expensive preparation, and takes all the well-known precautions and care which form a portion of modern antiseptic surgery; the results are good, and forthwith he sounds the praises of the proprietary disinfectant he has employed. Whereas if he had ordered an extemporaneous mixture, the results probably would have been better; or if he had substituted a little carbolized oil, or even alcohol and water, for the disinfectant, the results would have been as good. If the surgeon would make a scientific test of the antiseptic or antibacterial properties of the proprietary disenfectant, he would probably never himself use it, much less sign a certificate.

Exactly where this thing is going to end it is somewhat difficult to see. Like many other evils, it contains within itself the germ of its own destruction. It has already made the medical profession a laughing-stock among those who use the doctors to pull chestnuts out of the fire. It is, perhaps, not too much to hope that in a few years the people will generally recognize the folly of the profession, and the true worthlessness of these medical certificates will be so generally known as to deprive them of their force.

Meanwhile, we would advise those of our readers who desire to keep their own garments unspotted, and who dislike being made a tool of designing men, to refuse absolutely to sign any certificate of any proprietary medicine. Such signature does its writer no possible good in any way; it does the community no good at all. The only useful purpose it can serve is to enrich a man who is only a shade above the ordinary patent-nostrum vender. It may afflict and torment an already long- and much-suffering community. Cannot the average doctor get enough of manhood about him to say No even to a commercial traveller or bland sample-distributor?

YELLOWSTONE PARK.

OUR readers will remember that in our last issue we corrected some inaccuracies in our previous statements in regard to the chemical work of the United States Geological Survey. At that time we had not been able to get exact information; but through the courtesy of Dr. A. C. Peale, of the Survey, we are now able to make what the French call an "inspired statement."

Dr. F. A. Gooch spent the field season of 1884 in the Yellowstone National Park as a member of Mr. Arnold Hague's Yellowstone Division of the United States Geological Survey. Waters were collected from all the more important geysers, hot springs, and rivers of the Park, and were carefully selected with reference not only to their geological significance but also to the interest taken in them by the general public. Many of those forwarded to Washington were concentrated solutions, obtained by the evaporation of large quantities of the natural waters at the place where collected. From the end of the field season of 1884 until Dr. Gooch's appointment to the chair of Chemistry at Yale College, in July, 1885, work was carried on by him at the laboratory of the Survey in Washington, with the assistance of Mr. J. E. Whitfield, and a series of forty analyses has been completed, the results of which will be of great interest to geologists, chemists, physicians, and all interested in thermal waters. Analyses have been made of waters from the following geysers, viz.:

In the *Upper Basin*,—Splendid, Grotto, Beehive, Grand, Geauter's, and Old Faithful.

In the *Lower Basin*,—Fountain and Great Fountain.

In the *Norris Basin*,—Pearl and Fearless.

In the *Midway Basin*,—Excelsior.

Waters from the following springs are also included in the list, viz.: Artemesia, Bench, Hillside, Hygeia, Coral, Schamkessel, Cleopatra, Orange, Terrace, Soda Butte, Chrome, and Ink-Pot. The waters are of remarkable

complexity, and the results of the analyses are important, both from a practical and scientific point of view.

The fact that all the waters of the region contain arsenic in quite appreciable quantity is important from a medical stand-point.

PROFESSIONAL SECRECY.

THE question of professional secrecy has recently cropped up at Washington in rather a novel form.

Dr. A. Y. P. Garnett recently gave a certificate of disability to an employé of the Pension Bureau. For some reason the head of the Department demanded from Dr. Garnett a specific statement as to the ailment of the patient, which Dr. Garnett refused to give. An appeal was then taken to Secretary Lamar, who sustained the doctor. It is very curious to note the different legislation which exists in the various States in regard to this matter. In France, and we believe on the Continent generally, the secrets of the consulting-room are held by law to be inviolable. In New York the doctor cannot be forced by the Court to reveal any of the statements of his patients to him, although, if the consent of the patient is given in the court-room, the doctor is capable of acting as a witness. In this State—Pennsylvania—the law is silent upon the subject, leaving the doctor subject to the common statutes.

We suppose the time will come when in this and other States the professional secrecy will be protected.

Reports on Therapeutic Progress.

ON THE ANTAGONISM OF MORPHINE AND ATROPINE FROM A CLINICAL AND EXPERIMENTAL POINT OF VIEW.

LENHARTZ, of Leipzig, ventilated the question of the antagonism of atropine and morphine before the recent German Congress. In three cases observed by the speaker no such antagonism was evinced. The numerous cases of recovery after large and even enormous doses of morphine, without the aid of atropine, as well as the high doses of atropine usually thought indicated, speak against the antagonistic theory. Johnston (Shanghai) is a warm advocate of the doctrine. The explanation of the failures of the atropine exhibition after poisoning with morphine is, in Lenhartz's opinion, far from being satisfactory. Exhaustion and convulsions cannot be accepted as the causes of death, and the latter are much more frequent in his opinion (22.7 per cent.) than usually believed. Only in one-third of all cases does death take place here. It is, besides, singular that even minimal doses, such as ¼ to 1 mg., are supposed to have wrought a cure. From the very extreme limits of the dosation of the antidote, we can justly deduce the uncertainty of its indication. Johnston advises the exhibition of atropine in every grave case, even in presence of a weak, irregular pulse. Wood thinks the condition of the respiration ought to determine whether or not atropine should be given, while Binz positively condemns its exhibition in a small, weak pulse. Of one hundred and thirty-two cases of poisoning with morphine observed by Lenhartz, he treated fifty-nine with atropine, and lost twenty-eight per cent., while in the seventy-three cases treated on a different plan only fifteen per cent. died. He believes that atropine is directly injurious in morphine-poisoning. Binz recommended atropine solely on the ground of experimental researches; but it must be remembered that Binz produced merely sleep and never a grave intoxication in the animals experimented upon. Consequently Binz's results are not transferable to the field of therapeutics. Binz gave only 0.029 grm. to 0.05 grm. morphine per kilo.; Lenhartz saw recovery after 0.13 grm. to 0.28 grm. morphine. Animals in which a deeper intoxication had been induced did not perish through reduction of arterial pressure or the deteriorated respiration, but through the exhaustion of the nervous centres, resulting from the numerous convulsive and tetanic paroxysms. In eight instances the speaker exhibited atropine without producing the slightest effect; the animals died just as soon as before. Hence he opposes the practice of treating cases of morphine-poisoning with atropine.

The subject is sufficiently interesting to give space also to the discussion subsequent to the reading of Lenhartz's paper.

Freimuth (Danzig) reported a case of a simultaneous intoxication with morphine and atropine which recovered, and feels, therefore, justified in believing in the existence of a physiological antagonism between morphine and atropine.

L. Lewin (Berlin) defends the experiments of Binz. Even those opposed to the doctrine of antagonism, he said, admit the favorable

influence of atropine upon the damaged respiration. The clinical cases reported upon by Lenhartz have no value in Lewin's estimation. We cannot expect infallible recovery after the exhibition of an antidote, on account of the great diversity of the conditions of an intoxication.

Lenhartz concludes as follows:

1. Atropine appears to raise the arterial pressure in cases of poisoning with morphine only in some cases and only for a short time.

2. Binz recommends small doses of atropine.

3. An effective antidote should not only improve the condition of the patient but also remove the cause of death; hence atropine is no reliable antidote to morphine.

4. Washing out of the stomach in cases of acute intoxication with morphine should be the first and principal remedial resort.

SULPHATE OF SPARTEINE.

HANS VOIGT, working in Nothnagel's clinic, has come to the following conclusions respecting the therapeutical action of sulphate of sparteine. In small doses the salt increases the efficiency of the cardiac contractions and raises the arterial pressure. The number of heart-beats is always increased. These effects are observed within an hour of the administration of the drug, and continue for twenty-four hours. The author recommends the suspension of the administration of the drug for some days, but it may be given for a week without risk. The remedy does not always regulate the rhythm of the heart-beats. Its action on the respiration is variable. Diuresis appears to take place in proportion to the improvement of the cardiac action. A beneficial sedative action is often observed. Headache, vertigo, malaise, and other objectionable symptoms were but rarely met with as the result of the administration of small doses. The dose employed has been from 1 to 4 milligrammes ($\frac{1}{1000}$ to $\frac{6}{100}$ gr.). It will be remembered that in Sée's hands much larger doses, 5 to 20 centigrammes ($\frac{5}{10}$ to 3 gr.), were tolerated without cumulative or other objectionable effects.—*Lancet*, September 4, 1886.

HYDRASTIS CANADENSIS.

DR. WOLTERING, of Münster, confirms, in a paper published in the *Allgemeine Medicinische Central-Zeitung*, No. 47, 1886, the favorable results obtained with hydrastis canadensis by Fellner and other therapeutists of note.

The styptic virtues of the drug, especially in uterine fibromyomata, are very pronounced. Thus, Woltering saw cessation of the hemorrhages in three cases of fibromyoma with a uterus-length of 9 and 8 ctm. In one of the cases the uterus-length had at the end of one year decreased 2 ctm. True, besides the drug, the patient had had the benefit of long-continued self-tamponage and warm washes. The remedy was ordered in pill form on account of its unpleasant taste. Ten grammes of the fluid extract can conveniently be evaporated down to about three grammes of a dry extract, furnishing an excellent pill mass. Woltering orders usually as follows:

℞ Extr. hydras. canad. sicci, 6.00;
(To be prepared from fluid extr., 20.00.)
Extr. secal. corn.,
Ferri redacti, āā 3.00. M.
F. fil., No. 120.
S.—Two to five pills every three to four hours.

Of course morphine, aloes, or any other indicated remedy could be incorporated with this styptic. The above pills are taken readily and well borne, especially when taken during or after the meal. Woltering claims that neither wine, aromatics, nor liquorice cover the taste of hydrastis canadensis sufficiently.

In simple hemorrhagic endometritis the remedy renders also excellent service. In profuse bleeding from the lungs and stomach, however, a tablespoonful of oil of turpentine appears to be preferable.

THE TREATMENT OF EXCESSIVE SALIVATION DURING PREGNANCY.

Owing to the ordinary insignificance of this ailment, few medical works contain a lengthened account of it. DR. Y. SCHRAMM, however, had the opportunity, in the autumn of 1885, of observing a very severe case, and he has published some notes of it in a Scandinavian journal. Numerous remedies were employed with very little effect, for at first he imagined the salivation to be due to chronic mercurialization; but, later, he discovered that the patient was pregnant, and that pregnancy was the cause of the condition. Neither the iodide of potassium recommended by Le Maestre nor Von Ebstein's atropine treatment had any effect. Galvanizing the sympathetic also exercised only a very slight effect. Galvanizing the cervical sympathetic was resorted to with no perceptible result. Dr. Schramm then tried subcutaneous injections of pilocarpine

(as recommended by Von Labbe and Daveyeux). After seven injections of pilocarpine chloride, 0.01 gramme ($\frac{15}{100}$ gr.) per dose, there was a diminution of the daily quantity of saliva, as well as improvement in the general health of the patient. The excessive secretion, however, could not be entirely stopped. Bromide of potassium was tried, which acted quickly in the reduction of the flow; and Dr. Schramm prefers bromide of potassium to all other drugs, as it is harmless during pregnancy, and yet exercises a distinct effect on the salivary nerves, and on the origin of the facial nerve in the medulla oblongata, and also on the secretory filaments of the sympathetic. Professor Fleck made a chemical examination of the saliva, and found that ptyalin was absent.—*Brit. Med. Journ.*, September 25, 1886.

THE SURGERY OF THE PANCREAS, AS BASED UPON EXPERIMENTS AND CLINICAL RESEARCHES.

Dr. Senn continues his elaborate study of the surgery of the pancreas in the October number of *The American Journal of the Medical Sciences.* In connection with the subject of wounds of the pancreas, he strenuously maintains that complete extirpation of the head of the pancreas with the common duct is never justifiable, and that operations upon this portion of the gland for injury or disease, for the present at least, must be limited to partial excision of the head, with preservation of the common duct.

He finds that cirrhosis or chronic interstitial pancreatitis sometimes produces stenosis of the bile-duct or the pancreatic duct, and that, when the obstruction is followed by retention of the secretions, an operation becomes always necessary in biliary retention, which should be treated by establishing a new outlet for the bile into the duodenum, while the formation of an external pancreatic fistula in cases of cyst of the pancreas becomes necessary only when the presence of the swelling in itself has become a sufficient source of pain and discomfort to warrant treatment by abdominal section.

In pancreatic abscess he holds that a positive diagnosis of the presence and precise location of the abscess is only possible by resorting to explorative laparotomy, and that this should be always resorted to when the history of the case and the symptoms point to a probable diagnosis. The abscess found and located by abdominal section should be removed by partial extirpation of the pancreas when it is endopancreatic and located near the splenic end of the pancreas. When extirpation is impossible, or when it is located in the head of the pancreas or on the anterior surface of the pancreas, it should be treated by the formation of an anterior abdominal fistula; when located behind the pancreas, by through drainage, or lumbar drainage performed through the abdominal cavity.

The propriety of surgical treatment of pathological hemorrhage of the pancreas should only be entertained when the accident takes place in consequence of circumscribed, benign, pathological conditions, which in themselves do not jeopardize the life of the patient, and which admit of measures for arresting hemorrhage by direct treatment. Operative interference should therefore be limited to hemorrhagic cysts of the pancreas. In well-defined cases belonging to this group, it would be justifiable to resort to abdominal section as the only means of arresting fatal hemorrhage, by direct ligation of the bleeding points or by removing such localized portions of diseased tissue from which the hemorrhage has taken place.

A THERAPEUTIC SUGGESTION IN WHOOPING-COUGH.

Dr. Kohlmetz having passed through an epidemic of whooping cough, felt called upon to offer his therapeutic experience to the *Deutsche Medizinal Zeitung*, which published his communication on June 14, 1886.

The author dwells upon numerous remedies which he had tried without obtaining the slightest benefit. Among others, Kohlmetz mentions salicylic acid, which he applied to the nares in a number of cases, but soon abandoned on account of the inconvenience arising from this medication to the little patients. Quinine, given internally (according to the method of Dr. Sauerhering, of Stettin), gave likewise unsatisfactory results. Kohlmetz then tried the use of quinine in another form, which proved so eminently efficient that this mode of treatment deserves to be remembered by the practitioner. A solution of quinine is injected forcibly into the mouth, as far back as possible, against the posterior wall of the pharynx. To accomplish this end the tongue of a small child is well depressed by the mother with a suitable object, and a large child is made to pronounce the letter "a." The author declines to discuss whether the gratifying effects of quinine thus applied are due to its narcotic or antimycotic action. It

is certain, however, that the internal administration of the drug is not followed by such an improvement as is witnessed after this local application. Under this medication Kohlmetz noted in a great number of cases the whooping-cough to considerably diminish in three days, or, at the latest, in eight days. In the few instances in which the remedy was by the parents pronounced to be useless, Kohlmetz invariably learned that they, through motives of pity, had omitted the injections. In the majority of instances this procedure is not only well borne by the patient, but also openly asked for from the doctor. Kohlmetz's formula is as follows:

℞ Quinæ sulph., ʒi;
Acid. sulph., fʒss;
Aquæ dest., fʒvi. M.
S.—As directed.

During the first three days one syringeful is to be injected every two hours, and in the following four days every three hours. In very small children the quantity of injected quinine is to be correspondingly small.

Kohlmetz asks for his treatment further trials from other physicians in order to confirm his personal experience.

MANAGEMENT OF THE THIRD STAGE OF LABOR.

Dr. A. R. Fisher, assistant in the obstetric clinic of Professor Slavianski, in Kharkoff, has made a large number of observations with the view of determining the relative advantages in the third stage of labor of Credé's system of gentle manipulation or massage of the uterus, and the method of forcible expression of the placenta by powerful compression of the uterus. His statistics point decidedly in favor of the former method. In four hundred and ninety-three cases where Credé's method was employed, post-partum hemorrhage occurred twenty-five times, while in one hundred and eighty-three cases where expression was resorted to, it also occurred twenty-five times,—that is to say, the hemorrhage cases were five per cent. with Credé's method, and thirteen per cent. with the expression method. Again, the retention of pieces of membrane occurred less frequently with the former than with the latter mode of treatment, the percentage of cases in which this occurred being 5.7 and 7.4 respectively. The relative proportions of the occurrence of pyrexia, by which was understood any rise above 38° C., the temperature being taken three times daily, were 38.3° per cent. and 46.4 per cent., and the cases in which puerperal complications supervened were also fewer after Credé's method than after the more violent procedure, being respectively five per cent. and nine per cent. A diagram is given with the author's paper, which is published in the *Russkaya Meditsina* of August 10, showing that the process of the involution of the uterus is perceptibly quicker, at all events during the first week, in cases where Credé's method has been used, than in those where the placenta has been expressed. Dr. Fisher concludes with a general eulogy of Credé's method, as being the most rational and successful plan of treating the third stage of labor, and withal so simple that every doctor, and indeed every properly instructed midwife, should practise it habitually.—*Lancet*, September 18, 1886.

PERNICIOUS ANÆMIA AND BOTHRIOCEPHALUS LATUS.

Dr. Runeberg, of Helsingfors, speaking at the Berlin Congress (*Wiener Med. Blätter*, September 30, 1886) about the relations existing between pernicious anæmia and intestinal parasites, remarked that he had found bothriocephalus latus to have been the cause of numerous cases of pernicious anæmia observed in Finland. After removal of the worm, the affection, previously apparently incurable, disappeared spontaneously. Of nineteen cases of pernicious anæmia treated at the University Clinics of Helsingfors, Runeberg could trace twelve to the presence of the parasite. From 1878 to 1883—when the anthelmintic medication was introduced at the clinics—Runeberg lost nine cases of pernicious anæmia, while after that period of nineteen cases treated only one died. Runeberg advances the opinion that even other intestinal parasites, which were hitherto regarded as harmless, could produce similar results. Reyher has confirmed the observations of Runeberg, and published a pertinent paper some time ago in the *Deutsche Archiv für Kl. Med.**

THE TREATMENT OF PARONYCHIA.

Dr. Sellden writes in the *Eira* that he has for years made a special study of this subject. The greater number of his patients

* It is interesting to learn that Runeberg met with a good deal of opposition regarding this exposition of the etiological factors of pernicious anæmia.

have been miners, smiths, machine laborers, servants, and others whose fingers are exposed to injury. The disease commences in the subcutaneous tissue and spreads to the periosteum. There are differences of opinion as to the varieties of this disease, some authors asserting that there are four, others that there are only two,—the deep and superficial inflammation. Dr. Sellden, after a series of trials, found the following method most efficacious in the treatment of paronychia. When the patient will consent to an incision, the finger, after it has been opened, is instantly plunged in a tumblerful of hot water, which is then allowed to cool till it is nearly lukewarm. Half a teaspoonful of arnica is poured in, and a teaspoonful of the usual ten per cent. solution of potassium is added. This mixture is highly anæsthetic; the finger is held in it for from fifteen to thirty minutes, when the "bad matter" comes out. This expression is very characteristic of the phenomenon. The blood and pus exude in a thin stream about the size of a knitting-needle, which forms circles in the alkaline liquid, and finally settles in a thick mass at the bottom of the glass. Fifteen minutes or half an hour after the finger is dried it is rubbed with vaseline ointment containing ten per cent. of sulphide of potassium. The finger is then immediately enveloped in a poultice which continues warm till the next finger-bath, and thus hastens the cure. These finger-baths are taken from two to four times daily, and the wound is covered during the earlier days with sulphur ointment, and later with a boracic ointment. The finger is then bound up with a wadding compress and a bandage. Carbolic acid may be used in the finger-bath, but Dr. Sellden gives the preference to arnica, which he finds particularly useful in all sorts of injuries.—*Lancet*, August 28, 1886.

NOTE ON THE PATHOLOGY AND TREATMENT OF FEVER.

Dr. Finkler, discussing the subject of fever at the Berlin Congress, said that the modern views regarding regulation of heat and infection, and the great number of recently introduced antipyretics, justify a discussion of the origin and treatment of fever at the present time. We must admit that in fever an increased production of heat takes place, and that this is referable to a disturbance of the nerve-centres presiding over the regulation of heat. In other words, Finkler regards fever as a neurosis, and believes to have proven his assumption in Pflüger's (Bonn) laboratory. Fever is at the same time, in Finkler's opinion, a reaction, and, moreover, a salutary one.

In accordance with this view, the treatment of fever is to be a palliative one. It is safe to assert that the combination of baths and internal antithermic medication is the most successful plan of treating fever.

LANOLIN IN BLEPHARITIS MARGINALIS.

Dr. J. Herbert Claiborne states in the *Virginia Medical Monthly* for October, 1886, that recently he has been making extensive experimentation with lanolin in blepharitis marginalis.

At first it was combined with the yellow oxide of mercury, in the proportion of two grains of the yellow oxide to one ounce of the excipient. Some of the patients complained of its stinging considerably on application, and one patient who had a corneal infiltration in addition to blepharitis, stated that on putting a little between the lids, the pain was quite severe, and lasted some time. It was then prescribed pure for this patient, and in this form no pain or stinging followed its application. Two crops of hordeoma were developed under its use in one case, though the same patient had already had styes during the application of warm water. At the time that the styes appeared under the use of the lanolin no warm applications were being made. There was no visible improvement in any case, either combined with the yellow oxide or alone, and the eyelashes were invariably matted and stuck together by the salve, which seemed to cling quite closely.

There was no *uniform* complaint of stinging. Of two patients, one would almost invariably answer in the affirmative, the other in the negative.

From the above observations, the author concludes that the use of lanolin in blepharitis marginalis is without good effect, and, as long as we possess cosmoline, vaseline, and other excipients, its use is not to be advised.

TREATMENT OF DIPHTHERITIC ANGINA.

Dr. H. Roger and Prof. Peter, of Paris, give, in *Les Nouveaux Remèdes* of August 1, 1886, the following therapeutic hints in diphtheritic angina: In the benign form give in the beginning an ipecac emetic (3 to 12 grains of powdered ipecac in an ounce of the syrup of ipecac in tablespoonful doses) every

five minutes, until emesis is produced. Apply locally twice a day lime-water, or a solution of borax or alum, or, finally, lemon-juice. In grave cases repeat the emetic once or twice, apply four to eight times daily saturated lime-water, and touch the fauces with a solution of caustic soda (caustic soda and glycerin), or with a solution of nitrate of silver (nitrate of silver, 15 grains; water, 1 ounce). In the former case the patient ought to rinse the mouth with vinegar and water; in the second case, with salt water. In both light and severe cases tonics and supporting nutriments (bouillons, beef-tea, and milk) should be freely given.

During convalescence the authors advise change of air, continuance of the supporting alimentation, and wine of cinchona. In remaining diphtheritic paralysis the electric current is to be resorted to.*

SUCTION OF THE MALE URETERS.

Before attempting to excise a kidney for disease it is obviously of the highest importance to ascertain the working capacity of the opposite gland, for upon the power possessed by the latter of bearing the double burden depends the advisability of the proposed nephrectomy. The only method in which data as to this point can be obtained is by collecting the secretion directly from the supposed healthy kidney without admixture of urine from the diseased organ. This may be accomplished by the use of an instrument for tapping the ureters described by MR. FENWICK in the *Lancet*, September 18, 1886. The principle of its action is a slight suction force, which is exerted upon the ureteral orifice by means of a catheter and a small india-rubber ball. Notwithstanding the normal bladder seldom or never thoroughly contracts, the smooth muscle-wall relaxes sluggishly after its expulsive effort is over. This relaxation, at the same time that it produces a partial vacuum, diminishes the sphincteric contraction of the ureteral orifices, and any urine which may collect in those passages is partially propelled by the *vis a tergo* of the muscle of the ureters, and partially sucked by gravity and this negative pressure into the bladder. The aspirator consists of a small elastic ball and an oval glass bottle, which can be fixed to the end of a specially curved catheter, which latter is furnished with an elongated laterally placed eye. There are two catheters, one for each ureter. The bladder is first emptied and the catheter introduced. When it has fully entered it is rotated over, and at the same time steadied by the finger and thumb to the right or the left, according as the right or the left ureter is to be engaged. The laterally placed eye will now be exactly over the ureteral orifice, for the angle of the catheter is the same as the normal angle which the outer side of the trigone builds with the long axis of the urethra. Pressure of the right hand upon the elastic ball empties it of its air, and its stem is then affixed to the end of the catheter and the ball gradually released; the air in the catheter is correspondingly exhausted, and the ureteral orifice is sucked into the eye of the catheter, where it can be maintained by careful management of the ball or by continual counter-pressure of the finger in the rectum. In ten minutes sufficient urine will have collected in the catheter, for it rarely enters the glass bottle, to admit of examination. Mr. Fenwick states that whilst this mode of procedure is not infallible, it has rendered him great assistance in determining the health or disease of one kidney in cases where an operation for the removal of the other has been proposed.

ARE LUPUS AND TUBERCULOSIS IDENTICAL?

Our readers are aware that within the last two years the claim has been advanced that lupus is no disease of itself, as hitherto believed, but is, pathogenetically speaking, nothing but a tuberculosis of the skin. The acceptation of this view, which, we must admit, has been a rather general one, naturally was productive of radical changes in the treatment of lupus.

It is hence a matter of no small interest that at the recent Berlin Congress this view was declared to be erroneous by DR. SCHWIMMER, of Budapest. Let us briefly review the interesting discussion which subsequently arose, and in which men of wide reputation, such as Doutrelepont, Lewin, Lassar, Neisser, and Kaposi participated.

We follow the account given by the *Deutsche Medicinal Zeitung* of September 30, 1886:

Dr. Schwimmer holds that the proofs advanced in favor of the alleged identity of lupus and tuberculosis were too few and unsubstantial to overthrow the clinical evidences which he could present that the two diseases were non-identical.

* Henoch has in diphtheritic paralysis employed injections of strychnine with signal success.

The affection known commonly as tuberculosis of the skin is an extraordinarily rare one, while lupus itself occurs frequently. Among two thousand four hundred patients Schwimmer found ninety cases of lupus, but only five cases of pronounced tuberculosis of the skin. The clinical diagnosis in the latter affection is based on the bacterial proofs. In all of these five cases there was a rapid disintegration of the nodes. Besides, it is found that this affection invariably proceeds from the mucous membranes, while lupus appears on the epidermis, away from the mucous membranes, which, as a rule, this affection does not implicate. True, there have been observed cases of lupus on mucous membranes, but their number was very limited, and, besides, it is questionable whether the diagnosis was a correct one. Then the impression produced by the lupoid affection upon the general system is also to be taken into consideration. Tuberculosis, on the other hand, is a general disease, therefore it is not remarkable that lupoid patients sometimes become also tubercular. Hence we can look upon tuberculosis in a lupoid patient as a matter of mere coincidence. The author detailed two cases in which the clinical history went far in showing that the skin-affection present was lupus, and that the coexisting tuberculosis was an additional acquisition, having no relation with the lupus. Among ninety lupus patients Schwimmer found sixty-eight per cent. constitutionally untainted; in fifteen per cent. only he found a hereditary element. These facts speak against any etiological relation of the two diseases. Besides, it is at least singular that this relation, if actually existing, should not have been observed sooner. Schwimmer knows thirty-three cases of lupus, of which fifteen have for the last twelve years been under his observation, and of which not a single case showed the slightest symptoms of tuberculosis. As a last support of his view, Schwimmer advances the bacterial fact entering into the question. Many observers allege that in lupus but few, and but rarely, tubercle-bacilli are found. It is to be remembered that the tubercle-bacilli have a great resemblance to those found in lepra, both as far as form and chemical reaction go. Schwimmer shares the view of Prof. Babes, that the differentiation of the single bacillus is a matter of utmost difficulty.

Hence Schwimmer cannot, for all the reasons stated, be induced to look upon lupus as an affection either identical with or predisposing to tuberculosis.

As could be expected, Schwimmer's assertions met with a good deal of opposition from all quarters.

Doutrelepont (of Bonn) overthrew one of the evidences advanced by Schwimmer, viz., that fifteen lupoid patients had for twelve years shown no signs of tuberculosis, by remarking that tubercular processes do not take a uniform course. There are, he said, chronic tubercular inflammations of the knee, which proceed benignly, and others which take an opposite course. Similarly, a general tuberculosis need not be necessarily fatal, but may remain localized in one region for many years.

In forty cases of lupus the presence of the tubercle-bacillus was distinctly made out. Koch cultivated the lupus bacilli in sixteen generations, and found no differences between tubercle- and lupus-bacilli. Lassar, Lewinski, and Lewin, all of Berlin, equally opposed Schwimmer. Kaposi, of Vienna, alone upheld the latter's view. His remarks were introduced by what appeared an especial hit against the bacteriological school. "If the gentlemen," said the Viennese authority, "have microscopical evidences of the identity of lupus and tuberculosis, we cannot help that. But above all, the clinical features of an affection must be considered, for we are practitioners and not biologists. Otherwise we will be led towards false diagnoses."

Resuming, Schwimmer remarked he knew that he could hope for but few supporters of his views, and that all the opposing arguments heard could not induce him to abandon the former.

THE USE OF FRESH MILK IN THE TREATMENT OF ACUTE ARSENICAL POISONING.

Dr. Joseph Jones reports in the *Virginia Medical Monthly* for October, 1886, the notes of three cases of acute arsenical poisoning in which fresh milk given in large quantities proved of great value. The first of these cases consisted of an entire family,—father, mother, four children, and a young woman,—who were poisoned by arsenious acid thrown into a pot of soup. In these cases the excitation of vomiting and the use of large and repeated doses of fresh milk, followed by rest and milk and lime-water, resulted in the recovery of the seven individuals, In the second case, Dr. Jones was summoned to see a family, of whom he found the father lying dead on the floor of the front parlor, while the wife, son, and four servants were suffering from violent

vomiting and cramps. Investigation showed that white arsenic, which had been purchased to destroy vermin and rats, had been used by accident instead of white sugar in the preparation of pastry. The free use of fresh milk and milk and lime-water resulted in the recovery of the remaining victims. A third series consisted of a family who had been poisoned by arsenious acid ground up with the coffee. Here also Dr. Jones administered large quantities of fresh milk, followed by ipecac and warm water. Profuse and repeated vomiting was thus excited, and the stomach was thus washed out with effusions of ipecac and warm milk. As soon as the nausea had ceased the bowels were opened with castor oil. Here also all the patients recovered. Dr. Jones thinks that the milk acts by diluting the poison, enclosing it in its coagula, sheathing the inflamed surface of the mucous membrane, and when the system is capable of absorption and digestion, acting as an aliment of the greatest value.

DEEP INTRA-MUSCULAR INJECTIONS OF MERCURY IN SYPHILIS.

We abstract from the *Separat Abdruck aus der Allgemein. Wien. Med. Zeitung*, 1886, Nos. 32–34, the following conclusions of Dr. Schadeck (of Kiew) concerning the advantages of deep intra-muscular injections of mercury in syphilis:

1. The injection is easily and quickly executed, and is less painful than the ordinary hypodermic injection.

2. Abscesses are never formed.

3. Coagulations, nodes, and indurations do not occur.

4. The metal is quickly and completely absorbed.

As to the technique of these injections but little skill and experience is required. A canula about three inches long is fixed on a Lewin or Luer syringe, and the needle quickly and deeply pushed into the upper or middle portion of the gluteal region in an almost vertical direction. Then the syringe is emptied under an even pressure, the point of entrance pressed by the fingers for a few seconds, and then closed by adhesive plaster.

A NEW TÆNIAFUGE.

Dr. Numa Campi calls attention in *Il Raccoglitore Medico* (*Buffalo Med. Journal*, October, 1886) to the use of thymic acid, or thymol, in the treatment of tape-worm. This substance is derived from one of the *labiatæ*, or thyme, which is common throughout France and Southern Italy. The plant contains a volatile substance and liquid essence called thymine, which is isomeric with turpentine, which is also a tæniafuge. It is doubtless to this volatile substance that thymol owes its anthelmintic properties, although the tannin and a bitter principle are of importance. Thymic acid, or thymol, when perfectly pure, resembles camphor, but is much more transparent, and has a glassy appearance.

It is very slightly soluble in water, but can be readily dissolved in alcohol, in ether, and in alkaline solutions. It has an acrid, warm, somewhat persistent bitter taste, and the odor of thyme.

We know but little of its biological effects. Lewin, a few years ago, claimed that it had valuable germicidal and antiseptic properties, and was four times more powerful for those purposes than carbolic acid. Bucholtz has recently made a comparative study of its germicidal properties, and finds that a solution of 1 part to 2000 will prevent the growth of bacteria, being surpassed only by bichloride of mercury, while benzoin, carbolic, salicylic, and boric acids, creosote, the sulphates of quinine, copper, and zinc, and alcohol are all inferior in their antiseptic powers. The action of thymol, moreover, is not limited to micro-organisms, but affects the higher animals and man. Lewin discovered that frogs placed in weak solutions of thymol became insensible, and lost the power of reflex action to mechanical and chemical excitation, while the electrical irritability of the muscles and nerves remained intact. Leucocytes are rapidly attacked by thymol and lose their motility. From the description of the properties of thymol it is not difficult to conceive that it may be usefully employed in the treatment of tape-worm, especially since the experiments of Federici have shown that it is very effective for the destruction of the *Anchylostoma duoœnalis*,—the parasite which produces the terrible infirmity called Gottardo's disease, or miners' anæmia. But, in order to succeed with a remedy, it is not enough to know its physiological action, we must also know how to administer it; posology is undoubtedly the most important and the most delicate part of the practice of medicine. This simple truth, so generally recognized by medical writers, must be particularly regarded in the use of anthelmintics, the effectiveness of which depends so largely on the mode of administration.

Dr. Vanni, in the first case of tænia treated by thymol, gave 3 grammes (gr. xlv) in divided doses through the day, and 20 grammes (℥v) of castor oil at night, but succeeded in obtaining only five or six segments of the worm. On the following day, however, he administered 6 grammes (℥iss) of thymol, divided into twelve doses, to be taken every fifteen minutes, and at the end of the third hour the entire worm, including the head, was expelled.

Dr. Campi adopted the following method with his first case: In the evening he ordered 20 grammes (℥v) of castor oil to be taken fasting; in the morning he prescribed 8 grammes (℥ii) of thymol, divided into twelve doses, one to be taken every quarter of an hour; twenty minutes after the last dose had been swallowed, another 20 grammes (℥v) of castor oil were taken. A few minutes after a *tænia medio-canellata* three and a half metres in length was evacuated, the head being dead.

It is very important during the administration of the thymic acid to give the patient some cordial and stimulant; cognac and rum will be found suitable. Thymol is very depressing, according to the experiments of Husemann, and this will explain its action on the nervous system. Even with small doses the pulse becomes small and frequent, the respiratory movements slow, and the temperature is lowered. These effects, however, are easily and promptly counteracted by the use of stimulants.

Dr. Campi also calls attention to another fact. Husemann and Lewin say that thymol should be administered in small doses. On account of its caustic action, they claim that there is danger of producing digestive disturbances and gastro-enteritis. They recommended two or three teaspoonfuls of a one-half per cent. solution during the day. While it is true that we can never be too prudent in the practice of medicine, these doses are too fearfully homœopathic, when we consider that 8 grammes of thymic acid may be given in two hours and a half without producing the least gastro-intestinal disturbance.

The number of remedies recommended prove that none are reliable. The tæniafuges have left the physician and gone to the druggist, who sells his infallible vermifuges as he does his hair-restorers. We have only to ask any old practitioner about the use of anthelmintics to learn that this class of remedies is exceedingly unreliable. Most of these remedies, moreover, produce grave gastro-intestinal disturbances, and we are not surprised that Bamberger questioned which had done mankind most harm, the tape-worms or the tæniafuges. Dr. Campi recommends to the profession the use of thymic acid as a tæniafuge,—

1. Because this remedy, with the exception of a depressing effect, easily counteracted, produces no disturbance of the stomach or intestines.

2. On account of the rapidity and simplicity of its action compared with other remedies, which require *courses of treatment*, divided into three periods,—the preparatory, the expulsive, and the consecutive.

3. On account of the advantages it offers of being both a tæniacide and a tæniafuge.

4. Because in case of an error of diagnosis the remedy would produce an efficient purgation and disinfection of the alimentary canal.

5. Because it is reasonable to suppose that since thymic acid succeeds in expelling the *tænia medio-canellata*, the worm which above all others resists the action of anthelmintics, the same remedy will be of service in expelling all other varieties of flat worms.

6. Because we have, as yet, always been successful with the use of thymic acid, and have probably found a real specific, which, up to the present, did not exist.

GOITRE TREATED BY CARBOLIC ACID.

Dr. O. E. Haven states in the *Weekly Medical Review*, September 11, 1886, that he has met with a large number of cases of goitre in the public clinics in Chicago, all of which have been materially benefited, and almost all cured by the injection of 20 or 30 minims of a five per cent. solution of carbolic acid once or twice each week into the substance of the gland itself.

To do this thoroughly the needle of the hypodermic syringe is inserted one-half to one inch into the gland, and then the injection is made. Little or no pain is experienced, but only a feeling of dizziness, of which all the patients complain, and which passes away in a very few minutes. Usually this injection causes a contraction and a hardening of the connective tissue of the tumor and a gradual lessening of the blood-supply, and in the course of eight or ten weeks a complete disappearance. Out of the one hundred and fifty or two hundred cases treated in this manner, there were only two cases where any inflammation and feverish symptoms appeared, and in these cases they only lasted a day or two. The treatment is simple, almost entirely painless, as the smallest needle of the

hypodermic syringe can be used and is entirely effective. No other treatment can show such uniformly successful results.

A SPECIFIC FOR CONSUMPTION.

The long-searched-for infallible drug against consumption has at last been found.

We abstract the following from the *Der Fortschrift*, Geneva, September 20, 1886:

M. NAUDIN, member of the French National Institution, writes to the *Journal d'Hygiène* that mutisia viciæfolia cures consumption promptly and thoroughly. This statement is based on the experience of Dr. Sacc, who has been living for many years in Cochabamba, and succeeded in obtaining the stated plant—which grows in Bolivia, and is jealously guarded and kept secret by the natives—by trickery, flattery, and great presents. The plant enjoys the enviable reputation of curing phthisis and every affection of the respiratory apparatus. Dr. Sacc claims to have frequently found occasion to witness the verification of these statements regarding the plant, and does not doubt for a moment but this plant will in the shortest time acquire great celebrity. If these claims of Dr. Sacc are true, mutisia viciæfolia will become the greatest benefactor of mankind.

Detailed knowledge regarding this plant is as yet totally wanting. Mr. Naudin received thirty seeds of the plant, collected from about two thousand flowers, from Dr. Sacc. These seeds were planted in the Jardin des Plantes and in Algiers, and we will no doubt soon hear more about the plant. More important for the moment is the news that Dr. Sacc has sent to the Hospital for Consumptives at London and to various hospitals in Paris large quantities of an extract prepared from the plant. Thus the results will probably soon be rendered public, and we all trust that for the sake of suffering humanity these results will not fall short of his expectations.

THE ALTERNATIVES TO CRANIOTOMY.

From a paper with the above title, read by DR. ROBERT BARNES before the British Medical Association at its recent meeting (*British Medical Journal*, October 2, 1886), the author draws the following conclusions:

1. The legitimate aspiration and tendency of science is to eliminate craniotomy on the living and viable child from obstetric practice.

2. The advance of hygienic rule, the improvements in the forceps, in turning, in the induction of labor, and in obstetrics generally, have materially curtailed the field within which craniotomy can be justifiable.

3. In the most extreme degrees of pelvic distortion, where delivery *per vias naturales* can only be effected with doubtful success to the mother, Porro's operation is the legitimate alternative for craniotomy, it being understood that the opportunity of inducing abortion has gone by.

4. In less advanced degrees of pelvic contraction, but still incompatible with the delivery of a living child *per vias naturales*, the opportunity of inducing abortion having gone by, but in which craniotomy would effect delivery with strong presumption of safety to the mother, the Cæsarian section may be a proper alternative for craniotomy. This is the most debatable point.

5. In the minor degrees of contraction, say from three inches to three and a half or three and three-quarters inches, the opportunity of inducing labor having gone by, the far greater safety to the mother obtained by craniotomy, and the prospect of living children in future pregnancies by inducing labor, make craniotomy the proper course to adopt.

6. In other emergencies than deformity, as in obstructed labor from ovarian tumors, the alternative to craniotomy is to remove the tumor.

7. In cases of immovable tumors, Porro's operation is the proper alternative.

8. In rupture of the uterus, the child being delivered or not, Porro's operation is the proper alternative. There the interests of mother and child coincide.

9. In cases of disease or tumors of the uterus obstructing delivery, Porro's operation is the proper alternative.

10. In atresia of the cervix or vagina, Cæsarian section or craniotomy may be necessary; but incisions and gradual dilatation will more frequently be the proper alternatives.

11. When obstruction is due to hydrocephalus or dropsy in the child, embryotomy or tapping is indicated. When the child is dead, embryotomy is indicated, and decollation when the child is impacted and turning is hazardous.

12. In convulsions and hemorrhages, the proper alternatives for craniotomy are found in the more scientific methods of conducting labor under these complications.

Lastly, but the dream of Tyler Smith, the abolition of craniotomy will be fully realized

only when hygiene shall have triumphed over disease and deformity.

CHIONANTHUS VIRGINICA.

In *Les Nouveaux Remèdes* of August 18, 1886, we find a few descriptive remarks about *Chionanthus virginica*,—called in France "snow-tree," on account of its beautiful white flowers,—from which we condense the principal pharmacological points.

Medicinally, the bark of the root alone is employed in form of a fluid extract; saponine, discovered by Dr. Justice, probably represents the active principle of the bark.

The fluid extract is usually employed in the dose of ½ to 1 drachm, and is given two to three times daily for the sake of its aperient, diuretic, and alterative virtues. The drug is, besides, believed to act as a cholagogue, and to be of considerable value in jaundice. DR. J. A. HENNING, as stated in the THERAPEUTIC GAZETTE, indeed, regards the fluid extract of chionanthus virginica as one of the most eligible remedies in that affection. Although exercising a weak stimulating influence on the liver, Henning advocates its exhibition in cases of congestion of the portal vein. The drug is at the same time supposed to act as a stimulant to the lymphatic system, and to favor both diuresis and diaphoresis. In torpidity of the liver, Henning employs the drug, associated with podophyllin or leptandrin, as follows:

℞ Extr. of chionanthus, f℥i;
Podophyllin, f℥i;
Acet. of potassium, f℥ss;
Water, f℥iv. M.
S.—One teaspoonful every three or four hours.

The author adds that this treatment has never failed in cases of jaundice.

CURARE IN EPILEPSY.

In *La France Médicale* of September 14, 1886, we find a discussion of the therapeutic worth of curare in epilepsy which contains some noteworthy points.

BOURNEVILLE and BRICON, in a paper to the *Archiv de Neurologie* for March, April, and May, 1886, published their experiences with curare in epilepsy.

They commence by reciting the recorded experiences of numerous French physicians. Thus, Thiercelin (1861) obtained good results as long as the drug was continued. Bénédict (1866) found it to diminish the severity and lessen the frequency of the attacks. Runge (1879) obtained numerous cures by curare. Mundt (1866) and Voisin (1867), however, obtained no appreciable results.

Bourneville and Bricon having made sure that their curare was good, and by experiment having found the proper dose for a dog, decided upon giving to the human being a dose calculated in proportion to the weight of the individual as compared with that of a dog, giving an adult weighing seventy-five kilogrammes a dose of curare of 15 cg. (2½ grains). The solution given hypodermically was an eight per cent. solution. They treated thirty-three patients, including adults and children. The treatment was continued in some cases for more than six months, with the following results: One patient alone was said to be cured, one was distinctly benefited, and the third has a lessening of the severity of the convulsions.

The conclusion that the authors draw from their practice is that curare is practically useless in the treatment of epilepsy.

THE PATHOLOGICAL ACTION OF CORROSIVE SUBLIMATE

According to the Paris correspondent of the *British Medical Journal* (October 9, 1886), since corrosive sublimate (mercuric chloride) has been so generally used in surgery, and especially in obstetric practice, the attention of medical men has been attracted to the various intestinal lesions which have fallen under their notice. It has been asserted that mercuric chloride can produce deep lesions in the large intestines. Complex clinical facts may be interpreted in different ways, and have, therefore, not furnished convincing proofs of this assertion. Some authors suppose that, in cases of parturition where the sublimate has been used, the visceral lesions are due to the septic condition existing in the patient, and that the sublimate is not to blame. Experimental data, especially those obtained by Prévost's experiments, described in the *Revue Médicale de la Suisse Romande*, 1882, p. 553, indicate that visceral lesions often result from the effect of corrosive sublimate. Similar lesions to those observed in patients were experimentally provoked in animals; ecchymosis and hemorrhage, but neither gangrene nor ulceration were observed. MM. CHARRIN and ROGER used an aqueous solution of corrosive sublimate at 1 per 1000, and 1 per 4000,

which was injected under the skin or into the veins; this last method required smaller doses. After injecting 2 milligrammes under the skin of a guinea-pig weighing five hundred grammes, several ulcerated areas were observed; the injection of 5 milligrammes given in the course of eighteen days, or 3 milligrammes in six days, was powerless. These animals, experimented on by MM. Charrin and Roger, did not present during life any important symptoms of intestinal lesion; they grew thinner, had albuminuria, but rarely diarrhœa, and never intestinal hemorrhage. When the dose of mercuric chloride was weak, the animals did not succumb to it; they were killed at successive periods, in order to follow the course of the lesions. It was observed that the morbid appearances were localized in the large intestines, and especially in the ascending colon and cæcum; sometimes the ileo-cæcal valve and the terminal portion of the ileum were attacked. The earliest lesion consisted of small spots of hemorrhage dotted about on the intestinal mucous membrane, the peritoneum, the omentum, the outer surface of the kidneys, and especially the tissue of the lung. At a later stage these spots appeared as small ecchymoses, forming lines parallel to the axis of the intestines, varying in length from three to four centimetres. Later on, these ecchymoses became more extended, and the central portions sloughed away. A black eschar was thus formed, which gradually fell off and left an ulcerated spot. The contiguous portion of peritoneum sometimes showed signs of inflammation. Intestinal perforation has never been observed by these investigators. Thus the principal lesion which occurs after free administration of corrosive sublimate is apparently intestinal hemorrhage, which slightly tumefies the intestinal mucous membrane, and thus disables it as a factor in the process of nutrition. This hypothesis was verified by microscopical examination, which revealed the presence of spots of hemorrhage in the areolar tissue. The mucous membranes became detached and sphacelous without any glandular change taking place, as might be supposed would result from the elimination of the poison. These facts in experimental physiology applied to human patients indicate that a dose of 24 centigrammes of corrosive sublimate is necessary to produce intestinal ulceration in a man of sixty kilogrammes; but the conclusion is not applicable unless the sublimate be given in hypodermic or venous injections. It remains also to be proved whether human susceptibility to the influence of this substance is the same as that of the lower animals; some clinical facts suggest that smaller doses of corrosive sublimate act on the human subject and provoke intestinal lesions. Nevertheless, MM. Charin and Roger do not consider that the possibility of these accidents ought to be urged as a reason for proscribing the use of corrosive sublimate as an antiseptic agent.

THE TREATMENT OF DIABETES WITH PEPSIN.

In the *Indian Medical Gazette* for August, 1886, there is published a paper by the late SURGEON-MAJOR E. B. GARDNER as to the value of pepsin in the treatment of diabetes mellitus. About twenty-five years ago he commenced to prescribe it, and believes that he has obtained more advantage from its use than from any other single drug, although it should be combined with the standard remedies for this affection, and the well-known rules regarding special diet should be observed. He thinks that its use permits of some relaxation of the strict diet which it is impossible to continue if the disease is prolonged.

He reports a case in which, although not entirely cured, the sugar in the urine was reduced to one-third the quantity it was when he first came under treatment, while the patient was able to freely eat starchy and saccharine substances with comparative immunity, again an improvement over his previous condition, where before the slightest indulgence in sweet substances produced immediate and a large increase in the sugar, with attendant intense thirst. During the course of this treatment a cataract operation was performed on his right eye, and resulted in a perfect cure.

He administered 5 grains of pepsin three times a day. Its continuation for six weeks reduced the quantity of urine passed from thirteen pounds eight ounces to five pounds daily.

THE TREATMENT OF CHRONIC GONORRHŒA IN THE MALE.

DR. O. D. BALL, in a paper read before the Medical Society of the County of Albany, N. Y. (*Albany Medical Annals*, June, 1886), takes exception to the statement made by Otis that all cases of gleet are dependent

upon strictures, either of large or small calibre. Dr. Ball is thoroughly satisfied that a fair proportion of these cases have no stricture, at least none that will resist the passage of a sound the normal size of the individual's urethra. Of course where strictures are to be found they are to be treated, and the full calibre of the urethra restored before any other means for checking the discharge can be resorted to. Of all the various methods proposed to accomplish this Dr. Ball prefers incision for very many reasons, of which the greatest is the saving of time that it allows. In the method which Dr. Ball recommends for treating chronic gonorrhœa, the recognition of the diseased portion of the canal is a matter of considerable importance. This, according to Dr. Ball, can be determined by the character of the discharges quite as well as by a sound in a sensitive urethra. In a general way Dr. Ball states that we may say that the disease is mainly anterior to the bulb when the discharge is a thick creamy mucopus; when of a thin or viscid character it comes from the prostatic portion mainly. Having determined the locality of the inflammation as nearly as may be, Dr. Ball employs an ointment of oxide of zinc applied directly to the diseased membrane, using an olive-pointed bougie for carrying the ointment. The author has employed this method of treatment in fifteen cases, ten of which had contracted the disease six months or more before treatment, and in all cure was obtained. The average treatment of the cases was a little over four weeks. The longest in any one case which was under treatment was eight weeks; the shortest was ten days, except one case, who never saw any discharge after the first application was made. The formula which he employs is as follows: Zinc oxide, 3 drachms; lard, 3 drachms; simple cerate, 2 drachms. The method of using this ointment, which will adhere nicely to the bougie and yet be soft enough to apply itself to the urethral membrane, is to fill the constricted portion of the bougie out evenly and as smoothly as possible with the full calibre of the instrument, which may be lubricated with vaseline if the parts are sensitive. The bougie should be carried at once to the prostatic portion of the urethra as rapidly as possible, and then, after rotating it in both directions, it should be slightly withdrawn and then pushed back again, treating the remaining portion of the urethra in the same way. The patient should always empty his bladder previous to the application, and should be instructed to refrain from doing so again as long as possible. The application should be made at least twice a day.

GYMNOCALDUS CANADENSIS.

We abstract the following note about this drug from the September issue of *La France Médicale:*

The seed of *Gymnocaldus canadensis*, which is also known by the name of Kentucky coffee-bean, has the reputation of being a powerful toxic agent, resembling Calabar bean, with which it is allied. The watery extract of the drug modifies the sensibility considerably. Five minutes after a hypodermic injection of this preparation the frogs were insensible, and would not respond to irritation. With a moderate dose the sensibility of the conjunctiva remained, and on increasing the dose the anæsthesia became complete; perception and sensation became abolished. Motor troubles, consisting of general convulsive movements, the rigidity and subsequent paralysis of the muscles being complete, resulted. These phenomena had their origin in the central nervous system. The drug, besides, lessens the activity of the cardiac nerves, and acts as a stimulant to the pneumogastric nerve. The cardiac pulsations are diminished by one-half; and, finally, there is a lowering of the arterial tension.

THE USE OF SOME NEWER REMEDIES IN DISEASES OF THE UPPER AIR-PASSAGES.

At a recent meeting of the American Laryngological Association Dr. E. L. Shurley (*New York Med. Journ.*, September 11, 1886) read an elaborate report as to the results which he had obtained by the employment of a large number of some of the newer alkaloids and glucosides when used topically in diseases of the upper air-passages. As Dr Shurley's experience has been very large his results must naturally be of proportionate value, and we therefore think that we are doing our readers a service by presenting a full abstract of his conclusions.

Aconitine.—Dr. Shurley has records of twenty-five cases in which it was used either in the form of a spray or pigment. Two were cases of laryngeal phthisis; seven of chronic pharyngitis,—in five of which there existed acute exacerbation; eight of acute pharyngo-laryngitis; three of chronic follicular amygdalitis; three of chronic laryngitis; and two of pharyngeal neuralgia. The amount used each time varied from $\frac{1}{480}$ to

$\frac{1}{100}$ of a grain. In each instance there occurred in a few minutes the well-known physiological effects of tingling of the tongue and feeling of choking and numbness, to which were superadded, when applied in the nasal fossæ, lachrymation, with sneezing and vertigo. On two occasions these local symptoms were very severe and distressing to witness.

Only once did he observe a marked effect on the pulse immediately after the application. With a small dose—$\frac{1}{480}$ grain—these symptoms passed away in about two hours; with the larger doses from four to six hours elapsed before the patient became comfortable.

As to the local effect, in every case there arose in a very short time, almost immediately, an active hyperæmia. Even in the case of phthisis the pale color was deepened to a vivid rose, or a near approach to it. The ulterior effect was, in the acute cases, to cut short the course of local inflammation; in the cases of chronic pharyngitis, etc., there was no permanent effect, the cases of pharyngeal neuralgia—severe paroxysmal pain on one or other side of the pharynx—occurring respectively in a pregnant woman and in a woman suffering with supposed chronic inflammation of the ovary. The application of the pigment with a brush twice daily produced immediate and marked relief, although not a cure.

From these observations, therefore, he thinks that this agent as a local application was of little benefit, except in a few particular cases, while its very unpleasant effect upon the sensations of the patient was entirely out of proportion to its therapeutic value.

Agaricin (a glucoside from the *Boletus laricis*).—From the good effects in the abatement of nocturnal sweating which he has observed from the administration of this agent, Dr. Shurley was led to use it topically in a few cases of chronic nasal and pharyngeal disease, but, as he did not observe any physical or therapeutic effect, he has nothing to report at present.

Arbor Vitæ.—On account of its reputed efficacy in causing the disappearance of syphilitic warts, Dr. Shurley was led to use this drug as an application to intra-laryngeal papillomata. In one case, that of a boy 14 years of age, who was suffering with a progressive growth of these tumors on the vocal cords and ventricular bands, it certainly acted remarkably well. Only one of the growths—the largest—was removed by the forceps, the others disappearing under the use of a spray of the aqueous extract, applied once or twice daily, for a period of about three months.

In another case, in an adult, where there were five papillomata in the region of the anterior commissure of the vocal cords, this remedy only partially succeeded, the forceps having been resorted to for the larger ones. The treatment, however, seemed to prevent their renewed growth.

In still another case, one of cystic tumor of the right vocal cord, the arbor vitæ has had no effect whatever, unless, perhaps, to suppress catarrhal inflammation.

He has also used arbor vitæ in the local treatment of some cases of chronic laryngitis with benefit.

In preparing the fluid extract for spray, to render it unirritating it will be necessary to drive off nearly all the alcohol by means of heat, boiling it down to a syrupy consistence, and afterwards add water to the amount of one-third to one-half of the amount distilled off, then filter.

Ammonium Glycyrrhizate.—As a spray he has obtained no particular effects. Dr. Shurley prescribed it for a large number of cases of acute and chronic bronchial catarrh, in doses ranging between 2 and 5 grains. In the chronic cases it seemed inferior to ammonium chloride and ammonium iodide, but in the acute uncomplicated cases it has proved itself a valuable expectorant.

Cannabine Tannate (from *Cannabis indica*).—On account of the difficulty of making a solution of this substance, Dr. Shurley used it mostly in the form of powder mixed with starch, or in the form of an ointment with vaseline. He has used it in about fifty different cases, sixteen of which he has notes of, and a majority of these were cases of nasal or naso-pharyngeal disease. It possesses astringent properties, and is also a decided local stimulant. When applied undiluted to the nasal mucous membrane it causes considerable irritation, which lasts for a period of two to four hours, producing lachrymation and swelling of the turbinated mucous membrane. But when applied diluted with starch in the proportion of two to five grains to the drachm or with vaseline in like proportion, it has, as a rule, no such irritation, but a decided astringent, and afterwards soothing effect. It has proved a valuable agent, according to his experience, as a substitute for tannic acid, which latter he has ceased to use in the nasal chambers on account of its irritating effect; with children suffering from swelling of the nasal mucous membrane, either acute or sub-

acute, it will prove an excellent and efficient application, especially when mixed with vaseline.

Cadmium Sulphate.—This salt, which has been highly extolled as an astringent, is not superior to sulphate or chloride of zinc. Dr. Shurley used it as a spray and pigment in about thirty cases, in the proportion of from one-half to five grains to the ounce, and has noted no particularly beneficial result over zinc sulphate and chloride or silver nitrate, excepting in one case where the unpleasant results following the application of any solution of silver nitrate forbade its use. The cadmium certainly acted well, and seemed to bring about the desired result.

Cotoin (the active principle of coto bark).—Although there is not much literature upon this subject, it is reputed to be a good remedy for chronic diarrhœa and the diarrhœa of phthisis. Dr. Shurley administered it in two cases of diarrhœa of phthisis, but without signal success. He used it locally in about twenty cases of nasal and pharyngeal disease, twelve of which he has notes of. It is a powerful irritant to the nasal and pharyngeal mucous membrane—both healthy and diseased—and leaves no reactionary effect. Its best effects are obtained in cases of atrophic nasal and pharyngeal catarrh, and, when diluted in starch or sugar in the proportion of one part to three or four of excipient, it is, according to Dr. Shurley, quite equal if not superior to any other application—excepting galvanism.

Coniine Hydrochlorate (a salt of the alkaloid of *Conium maculatum*).—From the well-known selective action of conium on the muscular system and sympathetic nerve, one would suppose that this alkaloid—readily diffusible as it is—would act signally as a corrective in those local disorders of the throat and larynx characterized either by co-ordinate or ordinate spasm, etc.

Dr. Shurley has notes of ten cases representing various spasmodic actions of a pathological nature of the pharynx and larynx, one of which was an hysterical laryngeal cough, and one a local chorea, in which he applied this agent repeatedly in progressive doses (both by means of spray and pigment) without any sensible result in modifying the diseased action. He is therefore disposed to regard it as wanting in value for a local medicament.

Daturine (an alkaloid from *Datura stramonium*).—From the known value of stramonium as an anti-spasmodic, and its especial utility in the relief of spasmodic asthma, he was led to give this alkaloid a thorough trial as a topical remedy in affections of this sort.

He has notes of twenty-three cases subjected to experiment, as follows: 1, bronchial asthma; 1, spasmodic asthma; 2, periodic laryngeal congestion; 3, hay-fever; 1, chronic bronchitis (bronchorrhœa); 3, hysterical pharyngeal catarrh; 5, influenza; 1, laryngismus (with ovarian disease); 2, chronic naso-pharyngeal catarrh; 2, acute coryza; 2, chronic laryngitis.

The dose varied from $\frac{1}{100}$ to $\frac{1}{30}$ of a grain, and in some of the cases was repeated two, three, or four times a day; in each case, when the dose was large enough or frequently enough repeated, there appeared to the patient sooner or later the characteristic sensation of dryness of the fauces, etc. The patients with asthma were not sensibly affected by the treatment; two of those with hay-fever were very much benefited by the applications if repeated often enough (it was used in the form of spray), but the trouble was not arrested; three of those with influenza were decidedly relieved by three applications daily of a spray ($\frac{1}{100}$ of a grain), while the two others were not at all relieved. The neurotic cases were only relieved by a diminution of secretion, which, however, lasted but a few hours, necessitating a repetition of its use.

Ethyl Bromide.—This has been used almost entirely as an inhalation for the relief of cough. Dr. Shurley tried it in a large number of cases; it is not quite so efficacious as chloroform, but there is much less general anæsthesia from its continued use, and it leaves no unpleasant after-effects. He thinks, therefore, that it is deserving of more extended use for the relief of severe dry cough and spasmodic asthma.

Hyoscyine.—The hydrobromate is the salt which has been used. He has been unable to note any marked effect, proximate or remote, from the local use of this salt,—excepting in a few cases there arose a sensation of dryness which passed off in about an hour.

It produces no apparent change in the condition of the mucous membrane to which it is applied. The doses used varied from $\frac{1}{400}$ to $\frac{1}{100}$ of a grain.

Iodol.—Dr. Shurley used iodol in a few cases of ulceration of the mucous membrane, both specific and ordinary, and observed that it has nothing like the power to arrest the ulcerative process which iodoform possesses, although he cannot say but that slow improvement and healing followed its use. It

is sincerely to be hoped that this substance will prove to be a capable substitute for iodoform, because it is free from odor and causes no bad taste. He always applied it by insufflation.

Papain (papaw juice).—In one case of diphtheria he applied the fresh juice, and in six other cases it was used in the form of a glycerole,—either one-half or two-thirds strength; but no solvent action was observed on the exudate in any case. He adds that this was the experience of two other practitioners in his city, who also tried the remedy in cases of diphtheria. Therefore, so far as this limited experience goes, it has utterly failed to sustain its reputation as a remedy for the relief of real diphtheria.

Muscarine Sulphate (alkaloid of *Agaricus muscarius*).—Very little is known about this drug, but it is said to resemble in its physiological action that of physostigmine. Dr. Shurley applied it in doses of from $\frac{1}{30}$ to $\frac{1}{10}$ of a grain in twelve cases characterized by dryness of fauces and larynx, without any result excepting in two. One of these patients presented signs of great dryness of the fauces, and lack of buccal and salivary secretion, accompanying general nervous prostration; and the other had "dry catarrh" of the pharynx and larynx. In both of these cases a spray, repeated three or four times a day, seemed to afford great relief. The immediate effect was a pleasant tingling sensation, soon followed by comfortable heat and more secretion. No effect on the pupil was observed. In the other cases no immediate or remote effects, either objective or subjective, could be observed.

Physostigmine Salicylate.—This was thoroughly tried in a number of instances, of which he has six recorded. In but one instance did it produce any physiological action, or in any obvious way relieve the pathological condition of the parts. This case was that of a delicate young man who had from time to time suffered from spasmodic stricture of the lower pharynx and œsophagus; he experienced almost immediate relief from an application of $\frac{1}{120}$ of a grain, which lasted about six hours, when another application became necessary. At the end of forty-eight hours the attack ceased. Nothing which the writer had ever used locally in this man's case had acted so well as this agent in cutting short the course of the attacks.

Pilocarpine Hydrochloride.—Dr. Shurley has five recorded cases of pharyngeal catarrh characterized by a sensation of dryness and consequent distress in which he has tried this remedy, in doses varying from $\frac{1}{20}$ to $\frac{1}{4}$ of a grain, but without any positive result.

Piscidin (a glucoside from *Piscidia erythrina*).—Dr. Shurley has nine cases recorded in which this was used either as spray or pigment, in doses ranging from $\frac{1}{480}$ to $\frac{1}{24}$ of a grain. In only one instance could there be observed any proximate or remote effect, and that was a case of phthisis characterized by a very persistent and aggravated sensation of tickling referred to the pharynx. In this subject the application of a pigment in the proportion of $\frac{1}{120}$ of a grain to the drachm, every two or four hours, seemed to relieve the trouble in two days.

Resorcin.—Although attention was first called to this agent in 1860,—when it was presented as a supposed substitute for carbolic acid,—yet medical literature does not show that it has been at all extensively used until very recently. Dr. Shurley used it considerably during the past year, and believes it to be an important addition to the pharmacopœia. It is practically odorless and tasteless, while it is soluble in water; it is very feebly caustic, and, although a weak, still an efficient antiseptic. He has the records of thirteen cases in detail in which resorcin was used with good effect. Three were secondary syphilitic ulceration; one, tertiary; six, ozæna,—of which one was an aggravated case where the turbinated bones had disappeared; one, eczematous lupus of nose and pharynx; one, ulcerating and eczematous lupus; and one, typical herpetic pharyngitis.

The case of ulcerating lupus needs some additional remarks, inasmuch as it was a peculiar one; the subject, a child 9 years old, had been suffering for nine months; first with an obstruction of the nasal passages and an irritating discharge therefrom, followed gradually by the growth of fungous granulations in these passages and about the edges of the nares and upon the roof of the mouth (about the centre of the hard palate). When he saw her in February, 1886, there were extensive ulcerating granulations in the nasal passages and upon the surface of the roof of the mouth as well as at the margins of the nares; the nasal column was also ulcerated and its cartilage denuded. The patient's general condition was also much below par. Having anæsthetized her, he destroyed with the galvano cautery all the fungous tissue that he could reach and applied iodoform thoroughly. The iodoform was applied for

several days, as was also resorcin (in the proportion of one to four and one to six), but the main treatment afterwards consisted of resorcin used as a spray or pigment. The galvano-cautery was applied to two small spots in the mouth once since, but otherwise the treatment has consisted of the application of resorcin (and vaseline to moisten the parts) every day, conjoined, of course, with a recuperative and tonic plan of treatment. He saw the patient last on the fifth of this month (May), and observed that she was in good health, with very little deformity about the nose, and no reappearance of the trouble excepting at a spot situated on the inside of the cartilaginous nasal column. She is still under treatment. In the cases of ozæna, especially the one with the loss of the turbinated bones, the effects of this agent, used once or twice daily, as spray, were quite prompt, and it seems remarkable. The patient was under treatment about five weeks. He saw him about two weeks ago, and he told Dr. Shurley that he was comparatively free from trouble, and had been for the last three months; he uses a douche every day (as might be expected) of salt and water. Has no pain now and no more discomfort than is usual after such a lesion. The ulcerative process is apparently checked.

Sanguinarine Nitrate (a salt of the alkaloid of *Sanguinaria canadensis*).—No special results follow the local use of this drug, but for internal administration in a certain class of cases it is highly useful as a stimulating expectorant. He has noticed no unpleasant effects or depression from it in doses of $\frac{1}{16}$ to $\frac{1}{4}$ of a grain in syrup.

ICE-WATER ENEMATA IN TREATMENT OF DIARRHŒA.

Dr. Robert M. Simon, writing in the *British Medical Journal* for October 30, 1886, states that this mode of treatment has frequently been adopted in cases of collapse occurring during diarrhœa in young children at the Birmingham General Hospital. He recommends that ice should be dissolved in water and from two to three ounces injected. In his experience, which he says has been quite extensive, the immediate effect is good in producing sleep, as is important in the collapsed condition. Subsequently the effect upon the diarrhœa is also good, and it will rarely be found necessary to repeat the enema. Dr. Simon thinks that the cold enemata act by an astringent effect on the loaded vessels of the intestines, and so diminish the intestinal flux. It has sometimes been found to be expedient to give a few drops of brandy about the time of the injection, and perhaps internal medication should be continued. In his experience no depression or bad effect has ever resulted.

Reviews.

New Medications. By Professor Dujardin-Beaumetz. Translated by E. P. Hurd, M.D., etc. Parts I. and II. Detroit, Mich.: Geo. S. Davis, 1886.

So many new methods by so many men, new and old, crowd upon the practitioner, that he must either become a bookworm and abandon practice for reading, or, ignoring them all, allow his patients to suffer in the old-fashioned way. For such men nothing of late has appeared in print more useful than the condensed but practical and explicit work before us. Part I. contains, among others, chapters on "New Cardiac Medicaments,"—convallaria majalis, caffeine, and trinitrine,—with briefer mention of adonidine, sparteine, cactus grandiflorus, and cereus bonplandi. "New Methods of treating Stomach Diseases," which contain remarks upon gastrotomy, gastrostomy, and gastrectomy, the use of chloroform-water, carbon-disulphide-water, used in "lavage" of the stomach, forced feeding, or "gavage." Chapter iv., on "New Gastro-Intestinal Medications," treats of the application of electricity to stomach and intestines, of enteroclysm, alimentary lavements, anæsthesia by the rectum, and the treatment of hemorrhoids by hamamelis virginica. "Antiseptic Intestinal Medications" and "New Pulmonary Medications" complete the topics of Part I., while Part II. contains the pulmonary medications, treating of the mechanical means recently brought into use by the pneumatic cabinet, and by various modifications of its principles; in short, all the change in method and medicaments produced by the discovery of the bacillus tuberculosis are here studied, and the relative value of the different agents employed subjected to close scrutiny. "Antiseptic Pulmonary and Pleural Medication" forms the subject of two chapters, which are followed by "Antithermic Medicaments," new and old, by a chapter on the "New Hypnotics," chloral and paraldehyde, "On New Analgesic Medicaments," and "On Local Anæsthetics," the latter containing a close study of cocaine. The reader

can see from the above how large and important a field is covered by these two small volumes of the Physician's Leisure Library.

A Manual of Dietetics. By J. Milner Fothergill, M.D. Edin.
New York: William Wood & Co., 1886.

A really good book on dietetics would save more lives than a new work on therapeutics, but in the last few years the profession has made rapid strides towards scientific and rational feeding. The author says, "The day of dietetics has arrived," yet a careful reading of his pages will convince any one that there is, with much progress, much still to learn. We do not yet perfectly understand the intricacies of digestion, but each year we draw nearer to its sanctuary. We can now proceed on fixed principles, and at least formulate a truly scientific method of feeding. We may have to unlearn many things now considered as settled, and there is still a vast field for the explorer.

Dr. Fothergill is always an interesting writer. His style is captivating, and never more so than in the opening chapter on "The Objects of Food." Here one can see what unscientific feeding has been in the past ages of the world. In the chapter on "Forms of Food, with their Digestion," the author has embodied almost everything known on the subject, but in "Methods of Preparing Food" and "Condiments and Sauces" is contained the very rationale of cookery. "Condiments are curiously omitted from the dietary of invalids" is a sentence which deserves attention. The chapters on "Beverages" and "Stimulants" will attract alike the indulger and the abstainer, and contain a world of solid sense and carefully-digested experience. The chapters on prepared foods and artificial digestive agents will enlarge the knowledge of most readers considerably. The author expresses the wish that his knowledge of the prepared foods of the United States was as extensive as of those of Great Britain; and, *vice versa*, the medical man of average reading will find many preparations mentioned by the author by no means familiarly known to himself. Of peptonized foods the author gives a complete and satisfactory account. Following him as a guide, their use would be restricted to cases where they can be reasonably expected to be useful. He does not believe in "making a pauper of the stomach," by assisting it too soon or too long. The author gives careful directions for peptonizing milk gruel,—milk gruel soups, jellies, and beef-tea through the use of liquor pancreaticus,—and for peptonized enemata.

In Part II. we have chapters on "Food in Infancy," "Adolescence," "Adult Life," "Old Age," in "Acute Disease," "Convalescence," "Gastric Affections," in "Struma," "Anæmia," in "Constipation and Diarrhœa," in "Phthisis," in "Chronic Heart- and Lung-Disease," in "Bright's Disease," "Diabetes," "Gout," "Neurosal Affections," in "Obesity," "Indigestion," and "Biliousness," and in some other conditions, and the work concludes with a chapter on "Food given otherwise than by the Mouth." The reader can see what a wide field is covered, and while some of the subjects are treated briefly, they are treated carefully, as is the chapter on "Food in Obesity," which covers less than four pages, yet in that space we have not only an explanation of the reasons of obesity, but a careful description and analysis of the methods of Banting and Prof. Ebstein, with the author's comments on both. The chapter on "Gout" is thoroughly practical, and its material is very largely drawn from the author's own observation. It would form useful reading for many other than medical men. The author, however, leaves out of the question any consideration of gouty dyspeptics, as he would otherwise render his "chapter more complicated than desirable," and in doing so fails to show us how ably he might struggle with the complicated problem. Leaving out the gouty dyspeptic leaves out the greater part of the difficulty, and enables him to fall back upon the time-honored advice to eschew meat,—"No gouty man can drink malt and eat meat at the same time without paying the penalty." Of lager he speaks more favorably. Of wines, he reserves for the gouty "a poor thin wine, as the poorest claret or corresponding Italian," while spirits with water form "the best drink beyond doubt, and help to wash the uric acid out of the tissues." In the chapter on "Food in Diabetes," which the author distinguishes from simple glycosuria, giving his weighty reasons for so doing,—reasons which should be read and carefully considered by those who are ready to put their patients upon the rigid diet of the diabetic upon the first detection of sugar in the urine,—in this chapter the author raises many questions concerning the necessity for rigid diet. This is a thoroughly practical question, and very ably handled. "Is it, or is it not," he asks, "the fact that many persons fairly to be regarded as diabetics suffer much more from their diet-

ary than their malady?" In fact, he believes that during the quiescent periods of diabetes in many cases some relaxation should be allowed, and can be allowed with benefit rather than injury. In "Food in Constipation and Diarrhœa" the author says, "So long as animal broths are permitted so long will diarrhœa be intractable." Again and again, he remarks, has this been "driven like a spike" into his memory. Yet it is only a few years ago since beef-tea was the staple diet of typhoid patients, and in regard to the question of beef-tea the author is exceedingly plain and decided. In fact, the book is full of suggestions, and if not the ideal work on the subject, is the nearest to it in our knowledge, while the author's graphic style tends to fix his ideas upon the mind, and renders them far more valuable. In short, it is a work that all practitioners should read and reread, and endeavor to use in their every-day work.

THE GENUINE WORKS OF HIPPOCRATES. Translated from the Greek, with a Preliminary Discourse and Annotations by Francis Adams, LL.D., Surgeon. Two volumes. Vol. II.
New York: William Wood & Co., 1886.

To those who cannot read Hippocrates in the original Greek, and who are tormented with a burning desire to read him anyhow, this work will commend itself. Perhaps there may be in his writings much of practical and real value, but most of it has long since been incorporated in the works of modern authors. Possibly more is found in his pages than he ever dreamed of himself. The modern method of Biblical criticism applied to ancient medical works is capable of discovering almost anything the discoverer desires. But to those intending to settle in Greece, to engage in active practice at Parnassus or Olympus, the book will be invaluable.

In looking through this second volume, we at least carry away one fact. Hippocrates has protested against the modern heresy of homœopathy in no feeble words. "It is the business of the physician," he says, "to know in the first place things similar and things dissimilar;" and in his mind the "dissimilar" was of equal importance with the "similar."

Throughout the work the annotations of the editor are copious and wonderful; especially is this so in the "aphorisms." The more sententious the aphorism the more voluminous the annotation, and they at least serve to show us what the translator imagined his author to mean.

To those who are fond of ancient lore the work will undoubtedly prove a valuable and interesting possession.

MEDICINE OF THE FUTURE; AN ADDRESS PREPARED FOR THE ANNUAL MEETING OF THE BRITISH MEDICAL ASSOCIATION IN 1885. By Austin Flint, Sr., M.D., LL.D.
New York: D. Appleton & Co., 1886.

This address, never delivered as intended, has been published by the son of the late Dr. Austin Flint from his father's manuscripts, and now speaks to a larger audience than even the British Medical Association could have afforded. It is written in a happy vein, in its author's usual style, and deals, as its title imports, with speculation as to the future progress of our art. Of course no very pertinent prophecy could be anticipated, or if it contain such, time has not yet fulfilled it, but it is well worth reading, both out of regard to the rare man who wrote it, and for the interest which gathers about such speculations from a man so actively engaged in a practice of fifty years. Some of his words are words of warning, some of encouragement and hope, and over it all is a shade of sadness at the thought that the pen that wrote it, the mind that conceived it, are both at rest, and can take no part in that future progress of which he so hopefully wrote.

THE USE OF ELECTRICITY IN THE REMOVAL OF SUPERFLUOUS HAIR, AND THE TREATMENT OF VARIOUS FACIAL BLEMISHES. By Geo. Henry Fox, M.D., etc. The "Physician's Leisure Library."
Detroit, Mich.: Geo. S. Davis, 1886.

Hair tonics and restoratives which are not dyes have had their day, and here the hair-destroyer steps to the front, and immediately we are made aware of the immense number of individuals, mostly females, whose lives are rendered miserable by superfluous hair. It has struck us, since so many of these cases exist, that it may be possible that poor, blundering Nature grew these hairs where she did under the mistaken impression that they added beauty to the feminine lip, and that our Anglo-Saxon standard of feminine facial hairlessness might be a mistake. Of course, like every one else, we draw the line on chins anyway. One hardly could wish even an enemy's grandmother to have chin whiskers, and so the superfluous hair-destroyer is not in any case wholly superfluous. As a rival to the depilatory he deserves encouragement, for the depilatory takes skin and hairs and everything but bones, and can so easily be applied by the unscientific with such dire results that

the very difficulty of the electrolytic process is a point in its favor.

The complete method is given by Dr. Fox in his small but carefully-written work,—all the details, the difficulties, the failures. With the proper battery and instruments, which he minutely describes, and with a steady hand, good patience, and some practice, we imagine that most men can become successful superfluous-hair destroyers. We are waiting for the hair-planter next. E. W. W.

THE MODERN TREATMENT OF EAR-DISEASES. By Samuel Sexton, M.D.
Detroit, Mich.: Geo. S. Davis, 1886.

This book presents a classified list of over two thousand consecutive cases of ear-disease at Dr. Sexton's aural clinic, New York Eye and Ear Infirmary. It is intended to "bring into prominence the more practical features demanding treatment." "The cases have been divided, according to the anatomical location of the disease, into nine sections,—auricle, external auditory canal, drumhead, middle ear tract, complication of diseases of middle ear tract, mastoid process, important symptoms of ear-disease, neuroses, deaf-mutism." The author remarks in the introduction that a more complete classification will be found at the end of the book.

This book contains a fund of valuable information on diagnosis and treatment, not alone for the aural specialist, but still more for those members of the profession who, possessing some knowledge of aural disease, and daily meeting cases in practice, are a little at a loss at times, and while retaining the treatment in their hands, long for the more accurate knowledge of special study. There is nothing very technical or abstruse in the presentation of the cases, but they are very clearly described, very usefully classified, and in the appendix will be found a full account, well illustrated, of the methods, instruments, and formulæ used.

HOW TO CARE FOR THE INSANE. A MANUAL FOR ATTENDANTS IN INSANE ASYLUMS. By Wm. D. Granger, M.D., etc.
New York and London: G. P. Putnam's Sons, 1886.

The author of this little work has no new system for the management of the insane to introduce, no new theories to ventilate, but aims at supplying the intelligent attendant with a reliable and compact handbook, in which he will find directions for most emergencies and for the better conduct of his regular daily work. He gives a slight sketch of the nervous system, an outline of the generally accepted facts concerning mental diseases, and discusses insanity briefly, with its hallucinations, illusions, and delusions. The delusion, by the way, of thinking one's self a king or queen is one, according to the author, rarely met with in our free and favored land. "He does not expect such things among Americans." He probably expects his patients to imagine themselves ward politicians, rounders, and political bosses, or perhaps missing treasurers, bank defaulters, or delinquent city officials. Such are the beneficent effects of free institutions. Of moral insanity he says, "There are those who claim that the moral nature alone may be diseased."

"Many delightful friendships," he remarks, "are formed between attendants and patients." Probably those with homicidal mania form the warmest attachments, and "there is more appreciation and gratitude among the insane than there are contrary feelings;" but for all this we appreciate, we trust, the author's work, and recommend his manual as a book likely to be of use to those for whom he designed it. With how much deeper appreciation and gratitude the insane themselves would regard it we can form no estimate.

E. W. W.

A REFERENCE HANDBOOK OF THE MEDICAL SCIENCES, EMBRACING THE ENTIRE RANGE OF SCIENTIFIC AND PRACTICAL MEDICINE AND ALLIED SCIENCE BY VARIOUS WRITERS. Illustrated by chromolithographs and fine wood engravings. Edited by Albt. H. Buch, M.D. Vol. III.
New York: Wm. Wood & Co., 1886.

The late Dr. Austin Flint, in his "Medicine of the Future," said, "Voluminous encyclopædic works have probably had their day," and "there is reason for thinking that text-books which assume to extend over the whole domain of medicine will soon belong to the past;" but in spite of these wise words of a wise man, we think we see a wide field still open for such books as the "Reference Handbook," by no means a "handbook" in the sense of being small and convenient. It is so elaborate, full, and complete that one almost wonders whether, equipped with its series in full, the practitioner would need anything more. Certainly, when all the learning in these volumes has been assimilated and stored away in one brain, its possessor would be well provided for. The scheme of the work is very ambitious, and its execution commensurate with its editor's intention. Its

type is excellent; its illustrations profuse and apt; the articles written by men of authority and learning in their special lines at least. Take its articles on the "Forceps," on "Fractures," on "Fungi," as illustrations. Nothing more complete could be desired or expected. The article on "Fungi" especially is illustrated with chromolithographs of wonderful perfection, and will prove a true boon to the mushroom-eater, causing him to have one shudder the less as he eats his dubious meal. The anatomical topics, too, are extremely well illustrated, as also the section on "Hospitals, Hygiene, and Habitations," the latter giving the most perfect description of the various appliances for house drainage that we have ever met. In short, it is a work the first complete view of which will almost of necessity raise in the beholder the passion of possession. For in these busy times, when every moment is of value, when in want of information, there should be some reliable and ready method of obtaining it, and until the world grows slow again, we imagine that just such handbooks as this will be held in a very increasing esteem. E. W. W.

Correspondence.

LONDON.

(From our Special Correspondent.)

On the 1st of October medical life in London awoke from its pleasant autumn siesta. We are all back in town and hard at work again. Hospital practice has been resumed with fresh vigor. The medical schools are crowded with young and eager hearts, starting fresh on the course which many of their teachers would fain themselves again pursue. The societies have one after another had their opening night. It is at the societies as much as in the hospitals that a visitor among us can see the active spirits of our professional world; and it occurs to me that those of your readers who have not yet crossed the water may be pleased to hear something about these gatherings.

The most important of our professional societies is the Medical and Chirurgical Society, which is nearly as old as the century. This is the Royal Society of Medicine in England: possessed of convenient if not imposing rooms and a very fine library, and publishing an annual volume both of "Transactions" and of "Proceedings," which are highly valued. This society is supposed to attract the more elaborate papers produced with us,—papers that embody the results of research, of purely scientific bearing, or that do not readily fall under a clinical or a pathological heading. One hears complaints that "the Medico-Chi" is not sufficiently frequented by the younger men; but of this more anon.

Older even than the Royal Medical and Chirurgical Society, indeed a ripe centenarian, is the Medical Society of London, which, after a somewhat checkered career, has renewed its youth wonderfully within our recent memory, has built itself a handsome house, has reorganized its library, and has returned to fresh activity generally. The Medical Society is the least severely scientific of all our larger societies. It attracts many general practitioners to its meetings, and is thus a most valuable institution to the profession here, not only as being a social link between the family doctor and the consultant, the importance of which it would be difficult to exaggerate, but also because it introduces into our discussions the very kind of experience in which some of the members are comparatively wanting,—the experience of family practice. These remarks apply with even greater force to the Harveian Society, a small institution compared with those I have just mentioned, but one *par excellence* of and for the general practitioner, with a wholesome sprinkling of consultants to represent the hospital element. It is at the Medical and the Harveian that one may expect to hear real useful therapeutics discussed,—therapeutics stripped of the gloss of scientific novelty and put to the test of routine general practice.

Next to the Medico-Chirurgical in respect of scientific position come three societies which you may see regularly reported in our journals,—the Pathological, the Obstetrical, and the Clinical Societies. These are all very flourishing bodies, full of attraction to the more energetic spirits among us, publishing their respective "Transactions" every year. An evening at the Pathological is much affected by our youngest men. This is practically a society for the exhibition of specimens of morbid anatomy, with occasional discussions on subjects of pathology proper. The work of the Obstetrical Society corresponds so far with its name, but "gynæcological" would have been perhaps a more correct title. It fortunately attracts a fair number of general practitioners.

Lastly, we have the Clinical Society, which is of the most interest to your readers and myself in our present relation, inasmuch as it

was originally intended to be mainly a therapeutical society,—a society to which its members might bring for report and for discussion not only their observations but the results of their attempts at scientific treatment. It is comparatively a young institution, having seen but eighteen sessions. The Clinical was started as a kind of protest against the all-absorbing tendency of the Pathological Society. It was an attempt to bring us back to the consciousness that our business as doctors after all does not end with an attempt to lay a vast foundation of pathological science on which the medicine of the future may be surely built, but should be directed also to the relief of present suffering. You will agree with me that the following words of one of the greatest masters of modern English medicine, the late Sir Thomas Watson, which were spoken by him at the inaugural meeting of the Clinical Society, deserve to be disentombed from the first volume of the "Transactions," published in 1868:

"The society which we are founding tonight seems to me well-calculated gradually to bring about that which, in my judgment, is the thing most needful at present among us,—I mean, more exactness of knowledge, and therefore more direct and intelligent purpose, and more successful aim, in what is really the end and object of all our labors,—the application of remedies for the cure or relief of disease. Certainly the greatest gap in the science of medicine is to be found in its final and supreme stage,—the stage of therapeutics. . . . We have attained to a great degree of certainty in the detection and discrimination of disease in the living body. We know tolerably well what it is we have to deal with; but we do not know so well, nor anything like so well, how to deal with it. . . . To me it has been a life-long wonder, how vaguely, how ignorantly, how rashly, drugs are often prescribed. We try this; and, not succeeding, we try that; and baffled again, we try something else; and it is fortunate if we do no harm in these our tryings. . . . Of therapeutics as a trustworthy science, it is certain that we have, as yet, only the expectation. The influence of drugs upon the bodily conditions of health and disease is, indeed, most real and most precious to us; and some of them we have learned, in our contests with disease, to wield with much confidence and success."

It is wonderful how unwillingly we accept these unanswerable propositions at this moment in London. The *raison d'être* of the Clinical Society is only too clearly illustrated by its history. It constantly tends to relapse into a society for living *versus* dead pathology. Members bring their patients there, or read their cases there, but in the whole course of an evening you may never hear a word about treatment, always excepting the surgeons, who freely avail themselves of the opportunity to publish their novelties in the way of operations. This, for example, was the programme of the opening meeting on the 8th of October: A case of congenital malformation of the heart, with a discussion on the prediastolic murmur; a case of undeveloped sexual organs, with congenital defect of the tonsils; a case of pulsating tumor of the head, with Raynaud's disease; and further notes of a case of renal lithotomy. The truth is, that therapeutics is at a discount at this moment in England. The term "scientific therapeutics" is scouted by many here. Whilst your American journals, especially the THERAPEUTIC GAZETTE, are filled with papers of great value to the practitioner of medicine, many of our most active minds will not bend themselves in the direction of drugs. There are several reasons, as it seems to me, for this state of matters:

First, the problems of pathology are ever more attractive in such a place as London. Whether on the living subject or in the *post-mortem* room, "material"—as our German *confrères* rather irreverently call the *corpus vile* of the human frame—is ready at hand in abundance. Trained in the latest methods, our junior hospital men can quickly get together a case or a paper on a novel tumor or an anomalous eruption. It is a very different matter to undertake a laborious and faithful research on the value of a remedy, which may cost months of incessant labor, where a few imperfect observations may ruin a whole series, and where the result may after all be disappointing. Scientific therapeutics is not the kind of occupation to commend itself to us in these days of life-at-high-pressure. It is hardly necessary for me to add that the neglect of drugs has led to comparative ignorance of their action and uses, which, in turn, has begotten a widespread scepticism of pharmacology. I trust I may be forgiven for using the word "ignorance" when I explain that I refer to ignorance of the modern pharmacology of established remedies,—of the alkalies, iron, lead, antimony, opium, quinine, ergot, broom, etc. Men who have but little faith in physic do not trouble themselves to add to their ancient knowledge of these commonplace remedies,

and they are only brought into an attitude of attention when some new drug is introduced into practice. Their opinion of scientific therapeutics is founded on an estimate of the value of novelties, which are, alas, only too often of but evanescent reputation!

But we are not entirely without hope. I am reminded to-day of a quarter from which our favorite subject may look for the inauguration of a better order of things. The Harveian Oration was delivered this week on Harvey's birthday at the College of Physicians; and at this college there is a fair prospect that an effort will be made to follow the advice which their immortal Fellow left behind him,—to "search out the secrets of nature" by the revival of scientific therapeutics. Twelve months ago the then Harveian orator, Dr. Quain, lamented the decline of the healing art, and pleaded for the endowment of a therapeutical lectureship. The appeal did not fall on deaf ears. I have heard that the college has determined to apply at least part of the funds accruing to it under the Croonian trust to the endowment of an annual lectureship on some subject "in anatomy, physiology, or pathology, *with a view to the prevention, control, and cure of disease.*" You may smile at the amount of thankfulness which I express for so small a mercy as this clause extends to us; but I anticipate with confidence that this lectureship will encourage and foster that kind of practical therapeutical research in our hospitals the absence of which I have just lamented.

This letter would be incomplete without a few words about our junior societies. Within the last year or two we have had three hard-working societies on special branches of medicine started in London,—the Ophthalmological, the Dermatological, and the Neurological. These three bodies have these features common to them, that they "mean work," and that the members are more anxious to promote mutual improvement in the knowledge of the several specialties which they represent than to distinguish themselves publicly. I have little doubt they have a long and useful future before them.

"RHODAN."

To the Editors of the THERAPEUTIC GAZETTE:

GENTLEMEN:—I take the liberty of calling your attention to a little matter which once puzzled me a good deal, and I think may some of your other readers.

On page 536 of the August number is an abstract of an article on the action of *rhodan.* I think very few will know that *rhodan soda* (of the Germans) is nothing more than our familiar sodium sulphocyanide.

WALTER MENDELSON, M.D.

No. 209 WEST 46TH ST., NEW YORK,
November 9, 1886.

GRINDELIA ROBUSTA AND YERBA SANTA IN BRONCHITIS.

To the Editors of the THERAPEUTIC GAZETTE:

DEAR SIRS:—In looking over an old number of the THERAPEUTIC GAZETTE (November, 1885), I noticed a reference to an article I contributed to the *Medical News*, on the combination of the fluid extracts of grindelia robusta and yerba santa in bronchitis. Now, as there is a typographical error in the formula as given in your reference, and you have characterized the article as obscure, I would like to make the following explanations and correction through your valuable journal.

I have always found the following combination to produce the most salutary results in the first or pre-exudative stage of acute bronchitis, and to be especially effective in relieving the tickling, irritating cough which supervenes on assuming the recumbent position. I have used this combination for about five years in hundreds of cases with better results than I have been able to obtain from any other preparation. It is also valuable in the second stage of acute bronchitis and the chronic form of that disease; but here in our high altitude (six thousand seven hundred and thirty feet), at least, it exerts its most favorable influence in the first stage of the acute form of the disease.

℞ Fluidi extracti yerbæ santæ,
Fluidi extracti grindeliæ robustæ, aā ʒi;
Syrupi tolutariæ vel simplicis, q s. ad ʒviii. M.
Sig.—ʒi or ʒii every hour or two as needed for cough.

A more concentrated preparation than the above will cause nausea in certain cases, and not act more effectively than the weaker mixture above indicated.

Very truly yours,

E. STUVER, M.D.

RAWLINS, WYOMING TERRITORY, October 28, 1886.

PROPRIETARY MEDICINES.

To the Editors of the THERAPEUTIC GAZETTE:

GENTLEMEN:—This question is being freely discussed by the medical profession, and the

expression generally is in opposition to their manufacture and sale, except it be under certain restrictions or regulations.

That we may be the better prepared to apply the remedy to any evil, it is ofttimes necessary to know the cause that produces the evil, and in the consideration of this subject we think it very necessary to consider the cause that has led to, and now sustains, this extensive traffic, because it is sustained by the people, and there must be a cause for their having drifted into such an expensive and injurious habit.

In this, as in many other investigations, we cannot trace out one cause, but find many influences that tend to accomplish the same end, but a few stand out prominently and exert the controlling influence in establishing and perpetuating this traffic, which, if removed, would do much to banish a business which is so pernicious to the people and against the success of the true physician.

Polypharmacy has probably done more than any other one thing to create a demand and perpetuate a desire for proprietary medicines. The physicians who indulge in polypharmacy to the extent that their prescriptions contain from four to twelve agents, must of necessity have volume to obtain a compound that can be endured at all, and their patrons have come to look upon a large volume as being necessary. They look for it and expect it, and if they do not receive it they feel that they have not received the worth of their money, have put a commercial value upon the medicine, instead of recognizing the professional ability, and always contrast the bill with the sized bottle received, instead of the knowledge and skill manifested in diagnosis, prescription, and cure.

Empiricism, or routine practice, the twin brother of ignorance and stupidity, also forms an important factor in engendering and continuing this traffic. Ofttimes we find physicians who have a routine in practice. They give pills to physic, and quinine as a tonic after the patient has been "cleaned out" by the physic, or they have become so skilled in their knowledge of disease and the action of medicines that they can treat successfully a large per cent. of cases by their favorite compounds. They have an ague compound, a cough mixture, an alterative syrup, a cholera cordial, a liver pill, etc., and every person afflicted with ague, irrespective of age, sex, or condition, is prescribed for from the same compound, and each of the mixtures are dispensed in the same general way, until their patrons look upon them as medicine-venders, and again compare the amount of medicine received with the bill charged, and again with the amount that can be secured of the druggist for the same money, and find the amount to be greater, and therefore invest their money where they get the *largest* returns, thereby reducing the business of the physician to the plane of commercial exchange.

Prescribing proprietary medicines, or preparations so closely allied to them that it would require greater discriminating powers than is usually exercised to distinguish the difference.

The country is flooded with almanacs, receipt- and picture-books, containing laudatory accounts of the magic power of some "old and reliable remedy," or of some "new and wonderful discovery," that is so potent for good that many of the "eminent" physicians are prescribing it daily with marvellous success, and close with a number of certificates from the M.D.'s of the wonderful cures effected, thereby impressing the people that the practice is made up by prescribing such compounds, and again they are led to compare the amount prescribed with what they can obtain from the druggist for the same money, and estimate the curative art from a purely commercial basis.

How often do we find the physician inquiring through some medical journal the composition of some compound which he has been using, but of which he knows nothing, except it has been highly recommended, for the case in hand, and that results are not always satisfactory!

What we need to banish forever the baneful influence of proprietary medicines is a profession that will study to know the signification of certain pathological symptoms, and strive to know the actions of remedies singly, and search to know the relation of symptoms and remedies, thereby enabling them to meet certain symptoms with certain agents with as much certainty as we expect emesis after the administration of ipecac or lobelia, or catharsis after the administration of podophyllin or Epsom salts.

Prescribe singly, as far as possible, thereby learning the action of the various agents in the concentrations and attenuations, and learn what conditions can be met and relieved in the best possible manner in the least possible time with our various remedies.

Banish compounds from our shelves, and, if needed, prepare specially for the case, thereby teaching our patrons the important truth that no remedy, single or compound,

can be successfully prescribed in a "class" of cases known by a certain name, but that each case must receive its special treatment in accord with the special conditions that exist.

The American people are a dissipating people. They desire to work hard, late and early, physically and mentally, gormandize and engage in innumerable excesses, and yet be free from pain and enjoy the blessings of health; and if they do not, they hope to right all things by means of drugs, the satanic deception of which is found in the form of hypnotics in a large per cent. of the proprietary medicines, which, by their quieting influence, delude in the idea that restoration is being effected, forgetting the important truth which every physician should impress upon the minds of his patrons, that health can neither be perpetuated nor restored by means of drugs while living in open violation of the hygienic and dietetic laws governing health.

A. S. HUSTON, M.D.

PENDLETON, IND., October 29, 1886.

CONCERNING NOSTRUMS, ETC.

To the Editors of the THERAPEUTIC GAZETTE:

GENTLEMEN: — About semi-occasionally there is a howl sent up about proprietary articles, quacks, etc. Now, let us look at the subject of nostrums from a rational standpoint.

Scarcely a day passes but what the "busy practitioner" receives circulars for some wonderful preparation designed for the relief of suffering humanity: appended to this interesting pamphlet is a list of diseases in which this discovery "may be used to advantage" (that is, to the financial advantage of the proprietors); following this list will be found recommendations from physicians, many of whom occupy professors' chairs in medical colleges. How much they receive for their endorsement we humbler representatives of the profession, of course, do not know; but one thing is sure, and that is, that students, as a rule, look at their professors as being nearly infallible, and, accepting blindly their words as medical gospel, will naturally use these nostrums composed of—what? The manufacturers alone know, and they trust to the credulity of the profession for their income; they are sharp enough to put at the end of the circular in prominent type, "*Sold only on physicians' prescriptions,*" and also send their agents around to distribute samples; then, for fear the M.D.'s vanity will not be tickled enough, complimentary letters are written. This special mark of attention is simply another tickle under the ribs, causing an additional enlargement of the bump of "self-esteem" when we consider our importance.

The doctor "in ordinary," I am sorry to say, is not a reasoning animal, hence mental exertion is to be sedulously avoided; besides, there is Gull & Co., who manufacture remedies that will cure any human ailment: if the patient perchance die, why, "The Lord giveth, and the Lord taketh away," etc.

Now, the cause for the production of these nostrums, or "why is this thus?"

The majority of physicians talk of and prescribe for diseases as though they were entities, not pathological conditions.

The Indian supposes sickness to be the result of the presence of evil spirits, which must be exorcised. Not quite as crude, but equally as ridiculous, is the idea held by too many of our profession at the present day.

Now, let every physician, when called to see a patient, sit down and study the case before him, endeavor to discover the lesion, then do his own prescribing, using remedies whose value and composition are known and recognized, instead of the ready-made prescriptions put up by the considerate manufacturer, and whose real composition is veiled in obscurity.

Again, instead of trying to legislate against nostrums, etc., for the protection of the "dear people," let the doctor be what the name implies,—a teacher. Teach our patrons such simple hygienic laws as should be known by every one for the avoidance of the simple ills that result from their neglect. Show the public that we are not wholly selfish, and as we educate them to a right method of living, though at first it does not put as many shekels in our pockets, in the long run we will be the gainers,—for the reason that the sale of nostrums will be diminished, the respect and reputation of the doctor increased (and necessarily his practice), which should be synonymous with wealth.

Just as well legislate to make men honest as to stop the manufacture and sale of nostrums. Education alone will do the work, hence it is the duty of physicians to do their own thinking, which will of necessity compel them to fit the remedies to the patient, instead of the reverse; then will the manufacture of preparations "for physicians' use only" cease, but also the demand for those other nostrums which load the shelves

of drug-stores under the name of "Proprietary Medicines" gradually become extinct.

So long, however, as the physician, when a patient feels a little "off," tells him to go to the drug-store and get a box of "Carter's Liver Pills," "Castoria," "Winslow's Soothing Syrup," or a blood-purifier, conveying the idea, meantime, of disease as an entity, just so long will nostrums be put on the market and find a ready sale.

The profession must look among themselves for aid in abolishing this evil,—for evil it is,—not to the Legislatures of their States; for in other cases it has been pretty conclusively shown that prohibitory laws do not prohibit.

In fact, whenever the people are ready for a measure, then and only then will it be adopted, and until that time we are only "kicking against the pricks" in trying to gain legislative measures on this subject.

Now, instead of issuing a "legal boycott" on these manufacturers and their preparations, let us by study and application fit ourselves so that *we* are the best judges of what the patient needs, then give it; in this way the nuisance of nostrums will die a natural death of suffocation.

The physician should be a better judge of what is required by his patient than a man or set of men in China, Sandwich Islands, or any place, except at the bedside.

KENT O. FOLTZ, M.D.

AKRON, OHIO.

MEDICAL LEGISLATION.

To the Editors of the THERAPEUTIC GAZETTE:

GENTLEMEN:—It is my wish to make a few suggestions to Dr. F. T. Field, of Elroy, Wisconsin, and other of the readers of the GAZETTE who do, or who think they need, the protection of legislative enactments.

They should commence by instructing the people publicly and privately concerning proprietary medicines where nothing is or can be known about mixtures when no formula is given.

Physicians should not "hide their light under a bushel," but let it *so shine* that the people will not patronize proprietary mixtures.

If they wish to do anything in bringing about favorable legislation, they will have to commence long before the primaries by first selecting a man for the place who will do your work (he should be a good worker); then have an understanding among the physicians of the State or districts to work for the men so chosen.

If they (the physicians) have tact and will work together, they can nominate any man or men they wish. If a man is nominated who is opposed to your measure, commence talking against him to some of his warmest friends. He will visit you (for he cannot afford to have any influential physician against him). You can then make your demand, and, of course, he will promise to aid you. Have him put that promise in writing for use among physicians only, and, as a reminder to the Hon. Gentleman from Juneau County, a witness or two to that signature might be useful to strengthen the case.

If you ask for a law, do not ask for one unfair or exclusive, as you would fail to obtain it, which would serve you right.

If you do not succeed the first time you try, do not become discouraged and write to the THERAPEUTIC GAZETTE complaining of your ill success, but try again at the next session.

Have your bill ready, and have it introduced, read, and referred to the Committee on "Rights and Privileges." In the mean time write a letter, urging the passage of the measure, to every member of each branch of the Legislature with whom you are acquainted, and have all of your medical friends do the same all over the State.

If each member should receive half a dozen letters and as many petitions from his constituents he would think that law was in great demand, and he would not only vote for it, but would be anxious to do so.

I had a little experience a few years ago in getting an "Anatomy Law" passed in our very conservative State. I drew the bill and had it introduced, and to my surprise it passed the House of Representatives by a large majority, and lacked but one vote of passing the Senate the first round! The next Legislature I had it introduced again, and it passed the House but failed in the Senate. The third time I sent it to the member of the House from my own county, but he returned it to me with the assurance that "*it would fail.*" I sent it to a younger member, and it was introduced, when a physician, who had been a member at each of the three sessions, had it modified and passed, and it is the law today.

Our "Medical Practice Act," passed in 1855, came in the same manner, excepting it was worked upon for twenty years before it became a law.

Legislatures are very conservative bodies. They will not make radical changes unless

the demand for them is very urgent. Usually the worst foes to anything of the kind are physicians. They are the very ignorant ones or those who "stand high in the profession," and fear they will lose their standing if the ignorant ones are legislated out of business.

Suggestively yours,

I. B. WASHBURN, M.D.

RENSSELAER, INDIANA.

Notes and Queries.

TREATMENT OF ACUTE AND CHRONIC NASAL CATARRH.

DR. H. MARKS, of St. Louis, writes us as follows:

"I have a very good prescription for a snuff for acute or chronic catarrh, also traumatic rhinitis and acute coryza, especially when accompanied with pain of the nasal nerve. This formula effects a cure in twelve hours. It can be used as a snuff by the patient himself or in a powder-blower.

℞ Cocaine hydrochl., gr. x;
Ol. eucalyptus, gr. iii;
Iodoform, ʒi;
Milk sugar, ad q.s. ʒi. M.
Ft. triturate (snuff).

Sig.—Use every two or three hours. When relieved use two or three times a day.

"Another formula which I have found of service is a modification of that recommended by Beverly Robinson, which is as follows:

℞ Pul. fol. belladonnæ, gr. xx;
Cocaine muriate, gr. v;
Ol. rosæ, gtt. i;
Pulv. gum acaciæ, ad q s. ʒss. M.
Ft. triturate (snuff).

Sig.—Use with the powder-blower for anterior and posterior nares."

FLUID EXTRACT OF JABORANDI AS A LOCAL APPLICATION IN ECZEMA.

To the Editors of the THERAPEUTIC GAZETTE:

GENTLEMEN:—Having used fl. ext. jaborandi in erysipelas with good results, I have given it a trial in two cases of eczema, and have been so pleased with its action that I send brief reports of cases, asking others to make trial and report.

CASE I.—Male; aged 35; American. Each spring has an attack of acute eczema. Father subject to same. Eruption in patches all over body; intense itching. Gave internally liq. ars. et hyd. et iodidi, and ordered fl. ext. jab.; applied full strength to eruption. Itching relieved after first application. Recovered in a few days.

CASE II.—Physician; aged 60; American. Chronic eczema of the forehead of many years' standing. Skin very much thickened and cracked. Said he had tried everything he could learn of, and wanted me to see what I could do. Intense itching very disagreeable when air came in contact, so that he had to keep covered with a handkerchief. Applied fl. ex. jab.; full strength caused sharp burning for a few seconds. Reported in an hour he could stand contact of the air without discomfort. Continued application for several days, two or three times a day. Skin became thin; abrasions healed. Reports that he can only feel a slight discomfort when he becomes very warm. Feels so well he neglects to apply the remedy.

I report these two cases thus briefly, hoping others may more fully try and report progress with the remedy.

Respectfully,

G. W. ROBINSON, M.D.

TRINIDAD, COLORADO.

HELENIN.

Alant camphor ($C_6H_{10}O$).

The alant root (starwort) and extract drawn from it. Helenii had enjoyed for a long time the repute for being counter-irritants, and during the last few years helenin has been introduced. It is in form of colorless, neutrally reacting needle crystals. The melting-point is at 110° C. It is almost insoluble in water, but dissolves readily in hot alcohol, ether, and fat, or ethereal oils.

The views as to the value of this remedy were and are still divided. Merck, in his circular, calls attention to the substance, because it has lately again attracted the interest of physicians, as the increased demand and numerous literary publications abundantly prove. DR. KORAB deduces from the antiseptic qualities of helenin its power of subduing tuberculosis, malaria, and catarrhal diarrhœa. According to Dr. Korab, one part has the power to protect 10,000 parts of urine from putrefaction.

Dr. Valenzuela obtained excellent results in cases of whooping-cough. Chronic bronchitis always disappeared after the use of helenin. Dr. Valenzuela points out as constant symptoms after the application of the remedy, the abatement of cough, of asthma, and of pain in the chest; a narcotic effect helenin does not produce. It acts as a tonic

on the digestive organs, so that it may even be given to consumptives suffering from severe diarrhœa.

Dose, 0.01 grm. helenin ten times a day; of the tincture (1 to 5), three times daily, five drops.

In the department Gilette in the Hôpital Tenon at Paris there is an antiseptic dressing material used under the name "Hélénol du Dr. Korab," which very likely is a solution (alcoholic) of helenin.

CRYSTALLINE SALICYLATE AND SULPHATE OF PHYSOSTIGMINE.

These preparations, which enjoy the deserved attention of the medical world, do not require a general description. Of particular interest surely is the fact, recently proved, that eserine administered in very small doses against diarrhœa produces a remarkably lessening effect, while so far it was only known that larger doses act as purgative.

The fact that very small doses of eserine reduce the pressure of the blood, and larger doses increase it, is well known. This is based simply upon a lowering and raising of the activity of the heart. In connection with the laxative effect of the eserine prescriptions, the local action of the same agent upon the bowels has to be carefully considered. The peristaltic movement would be largely increased; the same may assume such dimensions that "cramp of the bowels" would have to be taken into consideration. Even after death this cramp continues for some time; the contractions connected with the same compress the intestinal vessels and expel the blood, so that the secretion ceases. Thereupon rests the recommendation of this remedy against cholera, but it seems improbable that a medicine of so diverging action should find extended internal application during an epidemic.

Attention is further called by Merck in his circular to the prescription of physostigmine against intoxication, with atropin and hyoscyamine, or daturine and duboisine respectively; dose for this purpose, 0.002 to 0.005 grm. subcutaneously. The advantage of this medicine is essentially in strengthening the contractions of the weakly throbbing heart.

In Mendel's *Centralblatt für Nervenleiden* a notice by Dr. Eschle recently appeared, which emphasizes the efficiency of physostigmine against diarrhœa.

Dr. Eschle has used physostigmine sulph. in a one-half per cent. solution subcutaneously, in doses of 0.001 to 0.0015 grm., and thereby stopped more or less severe diarrhœa. Larger doses (0.0025 grm.), however, show the known laxative action of physostigmine. Dr. Eschle specially recommends it in cases of dysentery, where it is important to check the bleedings which have such a weakening tendency and the albuminous defecations at least for some time.

SPANISH WINES.

La Vara de Esculapio (August 31, 1886) publishes an exhaustive leading article on the adulteration of wine in Barcelona, and gives an analysis of some "choice" ports:

Not alcohol	12.2 per cent.
Extractive	9 grammes per litre.
Ley	4.60 " " "
Sulphate of potassium	2.6 " " "
Alumina	0.180 " " "
Bitartrate of potassium	Normal.

The alcohol contained amylic spirit in large quantity. The other analyses were similar.

The named Spanish journal deserves every credit for the good work it is doing in exposing quackery and detecting adulteration.

GELOSINE.

According to the Paris correspondent of the *British Medical Journal*, October 9, 1886, at a recent meeting of the Société de Thérapeutique M. Guérin read a paper on gelosine, a mucilaginous substance extracted from a Japanese alga, which is sold in the form of dry whitish leaves. Gelosine is an excellent base, mixing easily with all pharmaceutical substances, soluble in alcohol and water, and in acidulated or alkaline water; it served as a medium for salts, powders, and different tinctures, and was of great utility in preparing suppositories. Gelosine gradually contracts and expels the water and medicinal substances it contains, which are thus spread over the surface of wounds or cavities in which it is placed in any form. Gelosine thus gradually regains its original volume, but can be used more than once for the purposes cited above. Gelosine, therefore, appeared to be preferable to liniments and ointments. M. Guérin showed specimens of cylinders and slabs of gelosine, containing camphor, creosote, sulphate of zinc, cocaine, tincture of belladonna, iodoform, corrosive sublimate, carbolic acid, and coal-tar. In order to use gelosine as a medium, the quan-

tity required should be immersed in its weight of hot water; the therapeutic agent is then added and incorporated. When the mixture becomes as thick as syrup, it is poured into moulds. These operations were completed in a quarter of an hour, and the expense was slight. Sterilized gelosine might be utilized in bacteriological research.

ANCHIETEA SALUTARIS.

The root of *Anchietea salutaris*, a violaceous plant, having a popular reputation in Brazil as a remedy in skin-diseases, has been recommended as useful in syphilis (*Chemiker Zeitung*, 1886, p. 619). The dose is 0.1 to 0.35 gm. of the powdered root daily, and the drug is also administered in the form of a syrup prepared from a tincture of the root. In consequence of the drug being used for syphilis and provoking a flow of saliva, it shares with some other substances the name of "vegetable mercury." Some years since DR. PECKOLDT called attention to the usefulness of the drug in skin-affections. He administered the powdered root, commencing with 0.35 to 0.70 gm. three times a day, and gradually increasing the dose.

The first effects are drastic, but these pass off after a day or two. Dr. Peckoldt isolated from the root-bark an alkaloid, which he considered to be the active principle, and named it "ancheitine."

NOTES ON SOME NEW DRUGS.

We abstract the following from *Der Fortschrift*, September 26, 1886:

1. *Ulexine* (seed of *Ulex Europæus*) furnishes an alkaloid called ulexine by Zerrard.

Physiological action: Applied to frogs the drug acts as a paralyzing agent.

Placed upon the tongue the hydrochlorate of ulexine causes anæsthesia, though to a less extent than cocaine.

2. *Cortex Cascara Amarga* (from the West Indies, from which Thompson prepared the alkaloid picramine) is said to be valuable against skin-diseases and syphilis.

3. *Jacaranda Lancifolia.*—In doses of 10 drops of the tincture and of f℥i of the extract the drug is claimed as a specific against gonorrhœa and syphilis.

4. *Njimo-Wood* (of West Africa) is regarded by the natives as a remedy against gastric pains and loss of appetite. Like the coca-leaves in Peru, the njimo-wood is used as a stimulant by the negroes.

5. *Radix Aristolochiæ Cymbiferæ* is said to surpass in energy the officinal serpentaria. In Brazil, the home of the plant, the drug is regarded as a sure cure for snake-bite and typhoid fever, and is called "mil homens."

6. *Radix Coca-Coca.*—The Indians in Peru use this drug against liver-affections and muscular colds.

7. *Radix Ping-Ping.*—Prof. Möller examined this drug, and states that it is valuable in vesical troubles.

8. *Radix Tayugæ.*—Stated to be of use in syphilis and skin-diseases.

9. *Semen Anda Assn.*—The oil acts like castor oil, but has the advantages of being a finer fluid and acting in smaller doses. Home, Chili.

10. *Semen Huingan*, Chili.—Used as an infusion against urinary affections.

LIATRIS ODORATISSIMA.

A frequent importation into the port of Hamburg during the past year has consisted of the stalks and leaves of the *Liatris odoratissima* (deer's tongue, or southern vanilla), which is said to be employed as a substitute for the more expensive tonka-bean in the scenting of snuffs. MESSRS. JEHE state that the quantity of cumarin in the drug is considerable, but it has never, in their experience, reached the amount mentioned by Wood, of two drachms to the pound.

EXCESSIVE SWEATING OF THE HANDS.

Excessive sweating of the hands is a source of much trouble, especially to dentists, whose hands of all things should be clean and free from the slightest odor. According to the *British Journal of Dental Science*, October 1, 1886, a mixture of salicylic acid or oxide of zinc with acacia powder will prove useful, although often boracic acid in saturated aqueous solution acts very well.

A NEW BITTER.

In a letter to the editor of *La Voz de Hippocrates*, DR. CRESCENCIO GARCIA recommends a Mexican plant, which is known popularly as "guachacata," as a substitute for quassia, gentian, etc. It yields a bitter principle to alcohol and boiling water. No analysis has been made of the plant, nor is its natural family named, yet the fact that it is largely used speaks evidently in favor of it and invites trials with the drug.—*Med. Press*, October 27, 1886.

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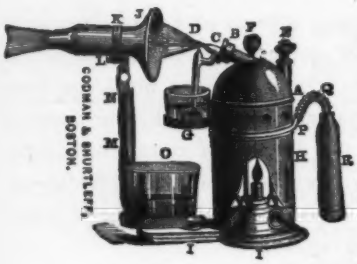

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Of BEEF TEA, DR. CHRISTISON says: "He "was able to obtain but a *quarter of an ounce* "*of solid residue in a pint.*"

This solid residue consists of, "besides the "trifling amount of proteid material and of "fat (which latter, in practice, is guarded "against with great care), *only the salts of the* "*muscle*, the *hematin*, and *allied pigments*, "*traces of sugar*, perhaps, *some lactic acid*, and "the nitrogeneous extractives *creatin* and its "*congeners*. As the original half pound of "muscle may contain about forty to sixty "grains of the salts, and ten to twelve grains "of the nitrogeneous waste products, the beef "tea (half pint) certainly contained no more." —PROF. BAUMGARTEN.

Of BEEF EXTRACT, DR. PAVY says: "There "are grounds for believing that a consider- "able proportion consists of products of "proteid decay, materials in course of retro- "grade metamorphosis, that are of no use as "nutritive agents."

The wellnigh superstitious ideas entertained by the *laity* of beef tea, are expressed in the allusion to the "*strength*" which is popularly supposed to be extracted in the *tea*; after which the *beef* is thrown to the dogs. The working man makes soup from a joint and consumes the "strength" and the *beef* both.

The medical profession insists that patients shall profit by the knowledge and progress of medical science, by the use of artificially-digested fresh milk, etc. The nostrum advertisers usurp the functions of the physician by prescribing fictitious "foods for invalids," foods which medical science has long since condemned.

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